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ANNALS OF PHILOSOPHY;

OR, MAGAZINE OF

CHEMISTRY, MINERALOGY, MECHANICS,

NATURAL HISTORY,

AGRICULTURE, AND THE ARTS.

BY THOMAS THOMSON, M.D. F.R.S. L. & E. F.L.S.

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ANNALS
OF
PHILOSOPHY.

INTRODUCTION TO VOL. XIII.

Historical Sketch of the Improvements in the Chemical Sciences during the Year 1818. By Thomas Thomson, M.D. F.R.S.

FROM the commencement of the *Annals of Philosophy* in the year 1813 to the year 1817, I inserted in the January number an historical sketch of the progress of science during the preceding year. This sketch was confined chiefly to chemistry, and the sciences connected with it. I noticed indeed the different branches of mechanical philosophy, and slightly glanced at some of the departments of natural history. But the primary object of the *Annals* being chemistry, and my industry being exerted to introduce into them every chemical discovery of importance, which came to my knowledge, in what country soever it had been made, I considered myself as pretty well qualified by the course of reading which the editing of such a journal naturally required to give a tolerably complete view of the progress of chemistry during the preceding year. Hence it naturally happened that these historical sketches often contained many important facts which want of room had prevented me from noticing in the previous numbers of the journal. I flatter myself, therefore, that they would be perused with some advantage by those of my readers who interested themselves in the science of chemistry, and who could not but wish to become acquainted with the various additions which it had just received. My sudden removal to the University of Glasgow, in Oct. 1817, laid me under the necessity of interrupting these historical sketches; and after a trial of two years, I find it difficult to resume them at the usual time; for in the months of October, November, and

December (or at least the last two of them), in which it would be necessary for me to be employed in drawing up the paper, I am almost wholly occupied in teaching, and could not, if I were to make the attempt, spare sufficient time for so laborious a task. We have, therefore, after much consideration, adopted a plan, which bids fair to improve the value of these papers, while it does not interfere with my duties as a professor of chemistry—at least so seriously. The plan is to publish two supplementary numbers, each to be prefixed to its respective volume. In the first, or July supplement, we propose to give an historical sketch of the progress of chemistry and mineralogy during the preceding year. This, I trust, I shall be able to draw up myself. The other supplement will contain a sketch of the progress of mechanical philosophy, botany, and zoology, during the preceding year, and will be drawn up by gentlemen well qualified to do justice to their several departments. Such is the plan which will be hereafter followed by the Editor of the *Annals of Philosophy*, and which it is hoped will meet with the approbation of its readers.

I. CHEMISTRY.

Several very important additions have been made to the science of chemistry during the course of the year 1818. In order to put my readers fully in possession of the facts, I shall be under the necessity of taking up some topics which came under our notice in the historical sketch printed in the *Annals of Philosophy* for July, 1818; but I shall take care to avoid all useless repetition. The advantages of arrangement are so obvious that I need make no apology for classing the different discoveries under their general heads.

I. LIGHT AND HEAT.

1. *Measure of Temperatures.*—All the precise notions respecting heat which we have acquired are derived from the use of the thermometer. The importance of this instrument has been long known, and much labour has been bestowed by philosophers in ascertaining the best way of graduating thermometers so as to make them comparable with each other. Now a thermometer is an instrument so contrived as to measure the dilatation of a liquid, and mercury has been found the most convenient liquid for the purpose. When heat is applied to mercury, it increases in bulk, and, rising in a graduated glass tube, indicates the degree of heat to which it is exposed. Several points remain still to be settled before the thermometer, even in its present improved state, can convey to us precise information. Do equal increments of heat occasion equal increments of bulk in mercury? Or do bodies expand more at high temperatures when they receive an equal increment of heat than at low temperatures? Or at what rate do they expand? These and several similar

questions remain still unresolved. The view which Mr. Dalton has taken of the thermometer and of expansion in the first volume of his System of Chemistry, has drawn the attention of philosophers to the subject, and seems to have led the Academy of Sciences of Paris to make it the subject of a prize, which was gained by MM. Dulong and Petit. A translation of their paper has appeared in our 13th volume. The experiments which it contains seem to have been made with much care; and are, therefore, calculated to decide our opinion respecting this very important but intricate subject. I shall endeavour to lay the facts which these gentlemen have established before my readers.

A preliminary point of some importance was the temperature at which mercury boils; or, in other words, what is the bulk of mercury when heated to the boiling temperature compared with its bulk at the temperature of 32°. Their mode of determining this point was exceedingly ingenious, and appears quite satisfactory. They filled a glass tube, shut at one end, and drawn out into a capillary point at the other, with mercury at the temperature of 32°. The tube thus filled was weighed, and the quantity of mercury which it contained determined. The tube was then kept in boiling mercury till it had acquired the temperature of that liquid; while the pressure exerted by the mercury in the capillary part of the tube prevented the mercury in the tube from boiling or any vapour from being formed. When the mercury was boiling hot, the capillary point of the tube was hermetically sealed by the blow-pipe, and the whole tube was allowed to cool. It was then weighed, and the weight of the mercury which it contained was ascertained. The comparison of this weight with that of the mercury at 32° gave the expansion of mercury at its boiling point; and knowing how much mercury expands between 32° and 212° it was easy to determine at what degree the mercury in a thermometer would stand if the whole of it were raised to the boiling temperature, and if the dilatation of the glass were abstracted. The result of the experiment made in this way is, that mercury boils when raised to the temperature of 360° centigrade, which is equivalent to 680° Fahr.

Another point of considerable importance, and without which indeed the boiling point of mercury could not be determined with precision, was to ascertain the absolute expansion of this liquid at different temperatures. The mode employed, though not altogether new, was, however exceedingly ingenious, and seems to have answered the purpose perfectly. It was founded upon the well-known hydrostatical fact, that if two liquids be poured into the opposite legs of an inverted syphon, the height of each will be inversely as its density. They filled an inverted syphon with mercury. One leg was kept at 32°, by being surrounded with a mixture of snow and water; while the mercury in the other leg was raised to different temperatures, by being surrounded with hot oil. The difference between the height of the mercury in the two legs was accurately measured at each temperature, and this difference indicated the specific gravity of the mercury in the hot leg of the syphon, or the expansion which it had sustained. The following table shows the dilatation of mercury for a degree centigrade at the various temperatures centigrade, indicated in the first column of the table, and measured by an air thermometer.

Temperature.	Expansion of mercury.	Temperature indicated by the dilatations of the ☿ supposed uniform.
0°	0	0·00
100	$\frac{1}{5550}$	100.00
200	$\frac{1}{5425}$	204·61
300	$\frac{1}{5300}$	314·15

From these experiments we learn, that if we employ an air thermometer to measure the temperature, and if we suppose the expansion of air to be equable, or, in other words, that equal increments of heat occasion equal increments of bulk, then mercury is more expanded by heat at high temperatures than at low temperatures, or its expansibility slowly increases as the temperature augments. This rate increases so slowly that between 32° and 212° it sensibly corresponds with the expansion of air; so that we may consider the expansion of mercury as equable up to the temperature of 212°. Between 212° and 392° there is a small increase in the expansibility. There is another small increase between 392° and 582°. The first of these increments, as may be seen by the third column of the preceding table, is equivalent to 4·61° of the centigrade scale. The second increment is equivalent to 14·15° of the same scale. The consequence of this is, that 200° centigrade on the air thermometer is the same as 204·61° on the mercurial thermometer, and 300° on the air thermometer the same as 314·15° on the mercurial thermometer.

As we have no other method of measuring temperature but the expansion produced, it is obviously necessary in the first place to fix upon some body as a standard by supposing its expansion to be equable. Our authors have made choice of *air.* They have taken it for granted that its expansion is equable, and have been induced in consequence to compare with it the expansions of all other substances. When we consider that it has been established by experiments which appear satisfactory, that all gases undergo the same change of bulk by the same increments of heat between 32° and 212°; and when we consider further the peculiar constitution of these elastic fluids, it will, I think, be acknowleged, that they are at least as likely, if not more so, to expand equably when heated, as any substances in nature; yet I think it a material defect in the paper of which I am at present giving an acount that a rigid examination of this point was not undertaken by the authors of it. A little consideration is sufficient to show us that the equal expansibility of the gases cannot be considered as completely established. If I recollect M. Gay-Lussac's experiments (for I have them not at present at hand to consult) they were carried no higher than the temperature of 212°. Mr. Dalton's were also limited by that temperature. Now it is obviously possible, as we see from the experiments contained in the paper before us, that the expansions of the different gases might have agreed with each other up to 212°, and yet have deviated from each other at higher temperatures. Thus the expansion of air and mercury follows the same law up to 212°, but at 392° a deviation is quite perceptible, and at 582° it has become considerable.

There is a method of deciding the question which has been long known to chemists, having been originally tried by Dr. Brook Taylor, and afterwards by Dr. Black, and by Dr. Crawford. I am rather surprised that such active experimenters as Dulong and Petit, who seem to have set out with the resolution of taking nothing for granted, did not have recourse to it. To determine whether equal increments of expansion were occasioned by equal increments of temperature, the philosophers above-mentioned mixed together equal weights of water heated to different temperatures, and observed whether the heat of the mixture was the mean of the temperatures of the two portions of water before mixture. Suppose they had mixed one pound of water at 40° with one pound of water at 100°, and that they found the temperature of the mixed liquid to be 70°, they would have concluded that up to 100° equal increments of expansion were produced by equal increments of temperature. It is well known that Dr. Crawford, from experiments made in this way, concluded that up to 212° mercury expands equably when heated. This conclusion is confirmed by the result of the experiments stated by Dulong and Petit. Now it would have been natural to have had recourse to a similar mode of measuring the expansion of air compared with the temperatures at heats considerably elevated above 212°. Water could not have been used for the purpose; but the fixed oils would have answered sufficiently nearly to the temperature of 600°, and mercury could have been used for still higher temperatures. Indeed a little ingenuity might have enabled them to carry the comparison up to a red heat by means of mixtures of lead or of tin; and thus to have settled a question which must still be considered as a desideratum of very material consequence, because it affects all our measurements of temperature, and all our conclusions respecting heat.

The dilatation of several solid bodies were compared by Dulong and Petit with those of air and mercury. The method was simple and ingenious. Having determined the absolute dilatation of mercury by heat, they measured the dilatation of it in a glass tube. The difference gave them the absolute dilatation of the glass. Every kind of glass tried was found to dilate the same. The dilatation of iron, copper, and platinum, was determined by fixing rods of these metals of known weights in the centre of a glass tube, shut at one end, and filled with mercury. The tube was then heated to different temperatures, and the expansion of the mercury ascertained by the quantity of it driven out of the tube. It is obvious that the volume of mercury driven out is equal to the dilatations of the mercury and the metal, minus the dilatation of the glass. The following table exhibits the absolute dilatations of these bodies.

Temperature centigrade.	Absolute dilatation of			
	Glass.	Iron.	Copper.	Platinum.
100°	$\frac{1}{6480}$	$\frac{1}{28200}$	$\frac{1}{19400}$	$\frac{1}{37700}$
200	$\frac{1}{6378}$			
300	$\frac{1}{6318}$	$\frac{1}{22700}$	$\frac{1}{17700}$	$\frac{1}{36300}$

2. *Dilatation of Steam and other Vapours.*—Last year has produced a valuable set of experiments upon the expansion of steam and the vapours of sulphuric ether, alcohol, naphtha, and oil of turpentine, when exposed to various temperatures. These experiments were made by Dr. Ure, of Glasgow, and have been published in the Philosophical Transactions for 1818. This subject had already attracted the attention of Gay-Lussac and of Dalton. But these philosophers did not carry their experiments beyond the boiling point of water. Dr. Ure's experiments were contrived with considerable ingenuity; and if they were performed with the requisite attention to precision, were calculated to yield results sufficiently accurate. His method was to confine a given bulk of vapour in the shut end of an inverted glass syphon of the requisite length. This end was surrounded by oil which was raised to the requisite temperature by means of an Argand lamp. Then mercury was poured into the open end of the syphon till the bulk of the vapour was reduced to its initial bulk. The height of mercury in the tube gave directly the elasticity of the vapour at the temperature observed. The great difficulty in experiments of this nature is to lute the vessel containing the oil round the glass syphon so as to prevent the oil from leaking out. There is also considerable difficulty in keeping the oil at a fixed temperature till the requisite quantity of mercury be poured into the tube. I instituted a set of experiments in somewhat a similar manner; but with a different object in view, a good many years ago. I found that tolerably accurate results were obtained when three persons were employed at once; namely, one person to regulate the temperature of the oil, and to ascertain the bulk of the vapour; one person to pour in the mercury into the open end of the syphon; and one person to

observe the height of the column of mercury. A fourth person sat at a table, and wrote down the temperature and the corresponding heights of the mercury. These experiments, like many others of mine which I had projected, and partly completed, were interrupted in consequence of bad health, and I have never found leisure or inclination to resume them. If they have been of no other use, they enable me at least to appreciate with tolerable accuracy the degree of precision to which experiments made in this way may be carried. The following table exhibits the elasticity of steam at different temperatures, according to the results given by Dr. Ure.

Temp.	Elasticity.	Temp.	Elasticity.	Temp.	Elasticity.	Temp.	Elasticity.
24°	0·170	165·0°	10·80	250·0°	61·90	292·3°	123·10
32	0·200	170·0	12·05	251·6	63·50	294·0	126·70
40	0·250	175·0	13·55	254·5	66·70	295·6	130·40
50	0·360	180·0	15·16	255·0	67·25	295·0	129·00
55	0·416	185·0	16·90	257·5	69·80	297·1	133·90
60	0·516	190·0	19·00	260·0	72·30	298·8	137·40
65	0·630	195·0	21·10	260·4	72·80	300·0	139·70
70	0·726	200·0	23·60	262·8	75·90	300·6	140·90
75	0·860	205·0	25·90	264·9	77·90	302·0	144·30
80	1·010	210·0	28·88	265·0	78·04	303·8	147·70
85	1·170	212·0	30·00	267·0	81·90	305·0	150·56
90	1·360	216·6	33·40	269·0	84·90	306·8	154·40
95	1·640	220·0	35·54	270·0	86·30	308·0	157·70
100	1·860	221·6	36·70	271·2	88·00	310·0	161·30
105	2·100	225·0	39·11	273·7	91·20	311·4	164·80
110	2·456	226·3	40·10	275·0	93·48	312·0	167·00
115	2·820	230·0	43·10	275·7	94·60	Another	exper.
120	3·300	230·5	43·50	277·9	97·80	312·0	165·5
125	3·830	234·5	46·80	279·5	101·60		
130	4·366	235·0	47·22	280·0	101·90		
135	5·070	238·5	50·30	281·8	104·40		
140	5·770	240·0	51·70	283·8	107·70		
145	6·600	242·0	53·60	285·2	112·20		
150	7 530	245·0	56·34	287·2	114·80		
155	8·500	245·8	57·10	289·0	118·20		
160	9·600	248·5	60·40	290·0	120·15		

Absolute precision would require a small correction in the above table for the dilatation of the glass tube. It is obvious that the capacity of the glass tube gradually increases with the temperature; so that the elasticities given in the table are a little below the truth; and the error increases with the temperature. The absolute expansion of glass, given in a preceding part of this paper, from the experiments of Dulong and Petit, will enable any person who is so inclined to apply this correction.

Dr. Ure's empirical formula for representing the elasticity of steam at different temperatures, which has been explained in the *Annals of Philosophy*, vol. xiii. p. 215, is very simple and ingenious, and must be of considerable use to engineers in calculating the force of steam at different temperatures. It is as follows: Let *n* represent the number of decades above or below

210° of the degree at which the elasticity of steam is required. Let r = the mean ratio between 210° and the temperature at which the elasticity of the steam is required. Then $28·9 \pm n$. log. r = log. of the elasticity required. Above 210° we add below 210°, we subtract n log. r.

The following table exhibits the elasticity of the other vapours examined by Dr. Ure.

Ether.		Alcohol, Sp. Gr. 0·813.				Naphtha.		Oil of Turpentine	
Temp.	Elast.	Temp.	Elast.	Temp.	Elast.	Temp.	Elast.	Temp.	Elast.
34°	6·20	32°	0·40	173·0°	30·00	316°	30·00	304·0°	30·00
44	8·10	40	0·56	178·3	33·50	320	31·70	307·6	32·60
54	10·30	45	0·70	180·0	34·73	325	34·00	310·0	33·50
64	13·00	50	0·86	182·3	36·40	330	36·40	315·0	35·20
74	16·10	55	1·00	185·3	39·90	335	38·96	320·0	37·06
84	20·00	60	1·23	190·0	43·20	340	41·60	322·0	37·80
94	24·70	65	1·49	193·3	46·60	345	44·10	326·0	40·20
104	30·00	70	1·76	196·3	50·10	350	46·86	330·0	42·10
2d	Ether.	75	2·10	200·0	53·00	355	50·20	336·0	45·00
105	30·00	80	2·45	206·0	60·10	360	53·30	340·0	47·30
110	32·54	85	2·93	210·0	65·00	365	56·90	343·0	49·40
115	35·90	90	3·40	214·0	69·30	370	60·70	347·0	51·70
120	39·47	95	3·90	216·0	72·20	372	61·90	350·0	53·80
125	43·24	100	4·50	220·0	78·50	375	64·00	354·0	56·60
130	47·14	105	5·20	225·0	87·50			357·0	58·70
135	51·90	110	6·00	230·0	94·10			360·0	60·80
140	56·90	115	7·10	232·0	97·10			362·0	62·40
145	62·10	120	8·10	236·0	103·60				
150	67·60	125	9·25	238·0	106·90				
155	73·60	130	10·60	248·0	111·24				
160	80·30	135	12·15	240·0	118 20				
165	86·40	140	13·90	247·0	122·10				
170	92·80	145	15·95	248·0	126·10				
175	99·10	150	18·00	249·7	131·40				
180	108·30	155	20·30	250·0	132·30				
185	16·10	160	22·60	252·0	138·60				
190	24·80	165	25·40	254·3	143·70				
195	33·70	170	28·30	258·6	151·60				
				260·0	155·20				
				262·0	161·40				
				264·0	166·10				

3. *Specific Heat of Solids at different Temperatures.*—That every substance has a specific heat peculiar to itself, or that every substance requires a quantity of heat peculiar to itself, in order to produce a given change of temperature, was first pointed out by Dr. Black. Experiments to determine the specific heat of bodies were afterwards made by Dr. Crawford and Mr. Wilke, by Lavoisier and Laplace, and more lately by Mr. Dalton. But it was not agreed upon, whether the specific heat of the same body remains permanent at different temperatures, or whether it does not vary according to some particular law. Dr. Crawford made a set of experiments to prove that it remains unaltered at all temperatures as long as a body does not change its state; and Dr. Irvine's theory of heat depended in a great

measure upon the accuracy of this opinion. Mr. Dalton, without any direct experiment, has taken it for granted as a consequence of his peculiar views respecting heat, that the specific heat of every body increases with the temperature; while Dr. Ure, from a set of experiments given in his elaborate paper on heat, published in the Phil. Trans. for 1818, concludes, that the specific heat of bodies *diminishes* as the temperature increases. Dulong and Petit, in their prize dissertation, so frequently mentioned in the preceding part of this sketch, have detailed a set of experiments made upon eight different solid bodies on purpose to decide this long agitated question. The experiments seem to have been made with great care. The method followed was the one originally suggested by Dr. Black, and followed by Dr. Crawford and Mr. Wilke. The solid substance was raised to the requisite temperature by plunging it in oil or mercury, and then suddenly immersed in so great a quantity of water that this liquid was only elevated a few centigrade degrees by the heat communicated by the solid. The water was contained in a thin vessel of tin-plate standing on four isolated points. In general, the water was cooled as many degrees below the temperature of the room as the solid was capable of raising it; and when this was not the case, a correction was applied for the heat dissipated during the experiment. Knowing the weights and temperatures of the solid and the water before the experiment, and the change of temperature produced on the liquid by plunging the solid into it, it was easy to determine the specific heat of the solid; that of water being as usual reckoned unity. But the accuracy of such experiments depends upon the precision with which the temperatures are determined. Dulong and Petit employed a thermometer capable of indicating $\frac{1}{100}$th of a centigrade degree, and of such a size as to give the mean temperature of the liquid examined. The following table exhibits the specific heats of the different solids tried at the different temperatures indicated by a centigrade air thermometer.

Solids.	Specific Heats.			
	Between 0° and 100°.	Between 0° and 200°.	Between 0° and 300°.	Between 0° and 350°.
Iron	0·1098	0·1150	0·1218	0·1255
Mercury	0·0330	..	0·0350	
Zinc..........	0·0927	..	0·1015	
Antimony.....	0·0507	..	0·0549	
Silver.........	0·0557	..	0·0611	
Copper	0·0949	..	0·1013	
Platinum	0·0355	..	0·0355	
Glass.........	0·1770	..	0·1900	

We see from this table, that the specific heat of each of these eight solids (unless platinum be an exception) increases with the temperature. Whether this increase of capacity be owing to the increase of dilatibility (the cause assigned by Dalton) cannot be considered as decided; but the experiments of Dulong and Petit, so far as they go, tally very well with such an opinion. Platinum increases the least in its dilatibility by heat; it undergoes also the smallest augmentation of its specific heat. The order of the increase of the dilatibility of the other metals tried is, mercury, copper, iron. Now these metals follow the same order in the increase of their specific heat. But it would be requisite to make a set of experiments on purpose on a greater number of bodies, and at higher temperatures, in order to obtain a satisfactory solution of this intricate question.

4. *Latent Heat of Vapours.*—It is universally known that Dr. Black first pointed out that when liquids are converted into vapours, a considerable quantity of heat becomes *latent;* and there could be no doubt, from a variety of obvious phenomena, that every vapour has a latent heat peculiar to itself. But though nearly 60 years have elapsed since the original discovery of this curious and important fact, I am not aware of any experiments to determine the latent heats of different vapours, except those by Mr. Watt to determine the latent heat of steam. These experiments have been lately printed; but as they are not yet, I believe, published, I do not consider myself at liberty to give an account of them here. Count Rumford also published a set of experiments, by which he ascertained the latent heat of steam and of alcoholic vapour. These latent heats are as follows:

Steam................	1040·8°
Vapour of alcohol, between	477·0° and 500°

In this deficiency of experiments, those who are interested in the subject lie under considerable obligations to Dr. Ure for a set of experiments which he has given in his paper on heat printed in the Phil. Trans. for 1818. His mode of proceeding was to distil a given weight of the liquid, the specific heat of whose vapour was to be determined into a receiver surrounded by a known weight of water, and to estimate the specific heat of the vapour by the increase of temperature which the water sustained. As no allowance was made for the heat dissipated during the experiment, it is obvious that the latent heats given by Dr. Ure are below the truth. Perhaps they will approach pretty nearly the true numbers if we increase them by about $\frac{1}{10}$th. The following table exhibits the results of Dr. Ure's experiments.

Steam..........................	967·000°
Vapour of alcohol..............	442·000

Vapour of sulphuric ether	302·379°
naphtha	177·870
oil of turpentine	177·870
nitric acid (sp. gr. 1·494)	531·990
ammonia (sp. gr. 0·978)	837·280
vinegar (sp. gr. 1·007)	875·000

The latent heats of the vapours of the last three liquids in this table are obviously composed of the latent heat of the acids, and the alkali supposed in a state of purity, and of the steam of the water with which these bodies are united in the liquids distilled. There is probably an error in the estimation of the latent heat of vinegar. The best vinegar that I ever met with contained only about six per cent. of acetic acid. Now I can hardly believe that so small a quantity could lower the latent heat of steam almost a ninth part. This surely could scarcely happen even if we were to suppose that acetic acid in the state of vapour has no latent heat at all.

5. *Laws of Cooling.*—Newton was the first person who gave a theory of the cooling of bodies. He took it for granted that the quantity of heat lost by a body in given small times was proportional to that which the body retained (considering the heat of the body to be the excess of its temperature above that of surrounding air). Hence it followed that if the times of cooling were taken in arithmetical progression, the losses of heat ought to form a decreasing geometrical progression. Trusting to the accuracy of this principle, he calculated the melting points of lead, tin, and various other bodies, by observing a red-hot piece of iron on which these metals were placed, and noting the times when they respectively became solid. When the iron became cold enough to admit the application of a thermometer, he applied that instrument, and measured the times of cooling, till the metal acquired the temperature of the surrounding air. He then calculated backwards to determine the initial temperature of the red hot iron, and the temperatures at which the respective metallic bodies attached to the iron lost their fluidity.

In the year 1739, Dr. Martine, of St. Andrew's, published a very ingenious paper on the heating and cooling of bodies, in which he showed, by a great number of experiments, made both by himself and by Muschenbroek, that the Newtonian law does not hold correctly; that bodies cool more rapidly than that law supposes; and that if it were rigidly accurate, hot bodies would take an infinite time to cool down to the temperature of the surrounding air. But a few years later, Kraft and Richman endeavoured to demonstrate the truth of the Newtonian law by experiments. And notwithstanding the various evidences that have been produced from time to time of the inaccurary of that law, it has continued to be admitted by chemists in general, and constituted the basis of the experimental investigations of

Irvine, Crawford, and even of Leslie. Dulong and Petit have at last undertaken to investigate the subject, and have published a most elaborate and intricate set of experiments, of which I shall endeavour to give an account here. I cannot say that I am quite satisfied with these experiments. They seem to me to want the requisite simplicity; nor am I quite convinced by the mode followed to establish some of the most important of the conclusions. I hope, therefore, that Mr. Leslie, or Mr. Dalton, will resume this most important subject, and endeavour to settle a preliminary question upon which the whole doctrine of heat, as far as it is the subject of experiment, in a great measure depends.

Dulong and Petit employed the bulbs of mercurial thermometers as the hot bodies, which they allowed to cool in different circumstances. They previously proved, by a set of experiments, that the law of cooling continues the same, though the size of the body to be cooled varies, and that alterations in the shape of the hot body are immaterial, as they do not affect the law of cooling. It would appear from their experiments that the velocity of cooling is nearly inversely as the diameter of the bulb. This had been stated by Newton to be the case in his *Principia*. And Dr. Martine had verified the law by a set of experiments, which, though not very precise, were, however, sufficiently so to show that the rate of cooling was sensibly as the law established by Newton.

They found that the law of cooling was the same when mercury, water, alcohol, and sulphuric acid, were employed. From this they have been led to conclude that all liquids cool according to the same law, and that the cooling of a liquid mass is subjected to the same law as a body of infinitely small dimensions.

But when the vessel employed to hold the hot liquid is varied, the law of cooling varies along with it. Thus the law of cooling of a tin plate was found to be more rapid than that of a glass sphere. This had been observed by Mr. Leslie, and led him to conclude that the law of cooling is most rapid in those that radiate heat least. Dulong and Petit assure us, that this is the case within the limits of the thermometric scale in which Mr. Leslie's experiments were made, but that the reverse takes place at high temperatures.

The first object to which our authors directed their attention, after these preliminary investigations, was the law of cooling in vacuo; and as this law varies with the surface of the hot body, they investigated it when the surface of the bulb of the thermometer was glass, one of the substances which radiates heat best, and when it was coated with silver, one of the substances which radiates heat worst. As it was impossible to make experiments in a perfect vacuum, they endeavoured to determine the quantity of heat which was carried off by the small residuum of air remaining in the balloon. This was estimated by means of a set of experiments made in air of different degrees of rarity, estimating the rate of cooling in each degree. Thus they were enabled to appreciate the portion of heat carried off by the small quantity of air remaining in the balloon, and hence to determine the rate of cooling in an absolute vacuum. I have no doubt that this method of proceeding will appear unsatisfactory to Mr. Leslie. He has given it as the result of his experiments, that what is called *radiation of heat* is merely heat carried off by the air. It follows as a consequence, I conceive, from this opinion, that in an absolute vacuum hot bodies would not cool at all. Dulong and Petit have not only admitted the possibility of their cooling; but have even calculated the law according to which they do cool in vacuo. Now as cooling in an absolute vacuum can only take place in consequence of radiation in the strictest sense of the word, it follows as a consequence, if their mode of reasoning be accurate, that heat is actually radiated from the surface of bodies, and not carried off, as Mr. Leslie supposes, by aerial pulses.

If the common notion of radiation be correct, it is obvious that the cooling of a hot body in vacuo must be the consequence of the excess of its radiation above that of the surface which surrounds the vacuum. It occurred to our authors to examine the rate of cooling, when the temperature of this surface was made to vary. From five sets of experiments which the reader will find given in the *Annals of Philosophy*, xiii. 243, it appears that the rapidity of cooling increases as the temperature of the surrounding surface increases. This seems to me a very extraordinary fact. I do not know well how it can be reconciled to the commonly received doctrine of radiation. I wish very much, therefore, to see these experiments repeated and verified. Our authors have expressed this very curious law in the following manner:

"The velocity of cooling of a thermometer in vacuo for a constant excess of temperature increases in a geometrical progression when the temperature of the surrounding medium increases in an arithmetical progression. The ratio of this geometrical progression is the same, whatever be the excess of temperature considered."

The law of cooling in vacuo, which our authors discovered by means of the experiments just alluded to, they express by the following proposition:

"When a body cools in vacuo, surrounded by a medium whose temperature is constant, the velocity of cooling for excess of temperature in arithmetical progression increases as the terms of a geometrical progression diminished by a constant quantity." And this law holds whether the surface of the cooling body be glass or silver.

If we were to suppose a body cooling in vacuo simply by radiation, and not to receive any heat by radiation, then the rate of cooling would follow the terms of a geometrical series;

or it would correspond with the Newtonian law. The reason why it deviates from this law is the quantity of heat which it receives by radiation during the process of cooling. This quantity must be constant if we suppose the temperature of the surrounding surface to be constant. Hence the reason of the constant quantity by which the geometrical series must be diminished. The reader will see by turning to the *Annals of Philosophy*, xiii. 245, how well the formula deduced from this law of cooling in vacuo agrees with the results of the experiments made by our authors on this subject.

Having thus determined the law of cooling in vacuo, or by simple radiation, the next subject of investigation was the law of cooling in air, or any other elastic fluid. It is obvious that the cooling in such cases is a complicated process. Part of the heat radiates from the body, just as it does in vacuo, and another portion of it is carried off by the conducting power of the elastic fluid. The effect of this last in cooling the body is easily determined by subtracting from the rate of cooling in the elastic fluid the rate of cooling in vacuo. The remainder obviously gives the heat carried off by the conducting power of the elastic fluids. This last quantity is not affected by the nature of the surface of the hot body, which is known to have so great an effect upon radiation. Our authors tried the cooling of one of their thermometers in air and in hydrogen gas, both when the surface of the bulb was glass, and when it was silver. The portion of heat lost by conduction was in both cases the same.

By another set of experiments, they have established that the rate of cooling, due to the conduction of elastic fluids alone, remains the same while the elasticity of the elastic fluid continues unaltered for the same differences of temperature between the hot body and the elastic fluid, whether the initial temperature of the elastic fluid be high or low. This law they have expressed in the following manner:

“ The velocity of cooling of a body, due to the sole contact of a gas, depends, for the same excess of temperature, on the density and temperature of the fluid; but this dependance is such that the velocity of cooling remains the same, if the density and the temperature of the gas change so that the elasticity remains constant.”

The effect of variations in the elasticity of the gas was then tried by cooling the thermometer in air and other gases of the elasticities 1, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$. From these experiments our authors have drawn the following conclusions;

“1. The law according to which the velocity of cooling by the contact of elastic fluids varies with the excesses of temperature remains the *same*, whatever be the elasticity of the air.”

“ 2. If the elasticity of the elastic fluid varies in a geometrical progression, its cooling power changes likewise in a geometrical progression; so that when the ratio of the first progression is 2, that of the second is 1·366, for air; 1·301 for hydrogen gas; 1·431 for carbonic acid gas; and 1·415 for olefiant gas."

It would appear from this that the cooling power of a gas is proportional to a certain power of its elasticity, which power is different for every gas. For hydrogen it is 0·38; for air 0·45; for carbonic acid gas 0·517; and for olefiant gas 0·501. Now as these last three numbers differ but little from 0·5, we may say that in the gases to which they belong, the cooling power is proportional to the square root of the elasticity.

If we reckon the cooling power of air at a given elasticity to be = 1, then the cooling power of hydrogen gas in the same circumstances is = 3·45, and that of carbonic acid gas = 0·965. But these numbers will change with the elasticity of these three gases.

I shall not attempt any further account of the results obtained by Dulong and Petit. The termination of their paper has been inserted so lately in the *Annals of Philosophy* that it must be fresh in the recollection of all my readers; and I find it very difficult to make their conclusions intelligible without introducing the algebraic formulas by which they have expressed them. This I have endeavoured to avoid for two reasons. First, because I wished to make the subject intelligible to those readers who are not acquainted with mathematics; and, secondly, because I could not well have given the formulas without introducing the tables on which these formulas were founded, which would have swelled this article greatly beyond the limits which I can spare for it.

6. *Production of Cold.*—It is well known that cold is produced by mixing together two solid substances, which, by their mutual action, are converted into liquids. During the liquefaction, the heat of liquidity is rendered latent, which occasions the cold. Hence the cold produced is limited by the latent heat of the solid body to be converted into a liquid; and this solid body is in all cases ice. Now the latent heat of water is not very considerable. The cold produced can never exceed it; and indeed from the obvious circumstances of the experiment, it never can even approach it. The latent heat of elastic fluids is much more considerable. When air is suddenly condensed to the fifth part of its natural bulk, the heat evolved is sufficient to kindle tinder; a temperature which must be higher than 600° of Fahr. When this air is suddenly allowed to expand to its original bulk, it resumes and renders latent all the heat which it had lost by compression. Hence if the temperature could be measured by a thermometer, it would be found to sink at least 600°. What a prodigious cold then would be generated by suddenly setting at liberty air condensed to the fiftieth or hundredth part of its original bulk? Gay-Lussac has proposed this as a method of producing cold without limit. There can be no doubt that the cold produced by this method may be increased without limit;

but we are afraid from the momentary nature of the temperature thus evolved, and the insignificant weight of air when compared with that of other bodies, that it would produce but very little effect on even the most delicate thermometer.—(Ann. de Chim. et Phys. ix. 305.)

7. *Melting Point of Bismuth, Tin, and Lead.*—The melting point of every particular substance has been hitherto considered as perfectly fixed, when other circumstances remain the same. Sufficient attention has not been paid to a fact which has been very often observed in water; but which appears not to be peculiar to that liquid. Water may be cooled down a good many degrees below the freezing point without congealing. I have sunk it nearly to zero by cooling it in thermometer tubes; but the instant it begins to congeal, it starts up to the temperature of 32°. In like manner, as we learn from the experiments of Mr. Crichton, bismuth always sinks 8° below its fusing point before it begins to congeal; but the instant the congelation begins, it starts up to its true fusing point.* Tin always sinks 4° below its congealing point, and starts up to it again the instant it begins to congeal. At the congealing point the thermometer remains long stationary, indicating that the congelation is going on slowly and regularly. Lead, on the other hand, does not sink at all sensibly below its point of congelation (*Annals of Philosophy*, xiii. 224). I consider this curious circumstance to depend upon the latent heat of these bodies. The subject is entitled to a much fuller investigation than it has yet received.

8. *Boiling Point of Liquids.*—It has been long known that when water is heated in a glass vessel it boils much less equably than it does in a metallic vessel. The temperature is raised a degree or two above the regular boiling point; then a torrent of steam rushes up through it, and the temperature sinks a little. This is repeated during the whole continuance of the process, and the temperature continues always vibrating between two points, distant two or three degrees from each other. If a few slips of platinum wire, or indeed of any other metal, are put into the glass vessel, these vibrations are prevented, and the water boils regularly, as it does in a metallic vessel when it has reached the boiling point. The cause of this difference has not yet been accounted for in a satisfactory manner. We owe some late ingenious speculations on the subject to M. Gay-Lussac, which I think it unnecessary to repeat here, as they have been inserted in the *Annals of Philosophy*, xii. 131, to which the reader is referred.

9. *Lamp without Flame.*—The only other set of facts con

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* Chemists are not agreed about the true melting point of this metal. Mr Crichton, of Glasgow, fixes it at 476°, which I am disposed to adopt from the known precision of this excellent artist. Berzelius makes the melting point 451½°; Thenard, 492°: and Gay-Lussac, 541° (Ann. de Chim. et Phys. ix. 308). This last number must be very erroneous.

nected with *light* or *heat* that it seems requisite to notice are some which originated from the curious discovery made some years ago by Sir H. Davy, that a fine platinum wire if plunged red-hot into mixtures of some of the inflammable gases and oxygen, or into mixtures of the vapour of alcohol or ether with common air, continues red-hot till the combustible bodies are consumed. In this case the combustion goes on without flame, sufficient heat being evolved to keep the wire red-hot. This curious discovery was soon after adapted to a common spirit lamp; and such lamps being at present quite common in this country, it seems unnecessary to give any description of them here. Davy has lately ascertained that the vapour of camphor may be employed instead of that of ether or alcohol for this experiment.

Mr. Dalton and Dr. Henry have examined whether in combustions carried on in this way the carbon of the alcohol be converted into carbonic acid, or into some gaseous compound, containing less oxygen. The result of their experiments was, that no other compound of carbon, except carbonic acid, was formed.—(*Annals of Philosophy*, xii. 245.)

II. SUPPORTERS.

Very little addition has been made during the course of 1818 to the knowledge which we already possessed respecting these bodies. The only new facts that I am aware of are, one respecting *iodine*, by Lampadius, and a numerous set of experiments on *cyanogen*, by Vauquelin. This substance, though not strictly speaking a supporter of combustion, yet seems to be entitled to a place very near them, as it unites with hydrogen, and forms with it an acid, as is the case with all the supporters, except oxygen. I am induced on that account to place the new facts discovered respecting it by Vauquelin under the title which stands at the head of this paragraph.

1. *Iodine.*—Lampadius has ascertained that iodine dissolves with great facility in sulphuret of carbon, giving it a dark reddish-brown colour. One grain gives a deep tinge to 1000 gr. of the liquid.

2. *Cyanogen.*—Water, as is well known, dissolves about four and a half times its bulk of cyanogen gas. Water thus impregnated is transparent and colourless, has a strong and peculiar smell, and possesses acid properties. Vauquelin has ascertained that when this liquid is kept for some time, the cyanogen and a portion of the water mutually decompose each other. The water becomes brown coloured and deposits some brown flocks, and it is found to contain three new acids, each of which is saturated with ammonia. These acids are hydrocyanic acid, carbonic acid, and a new acid, which Vauquelin proposes to call cyanic acid, because he thinks that it may be a compound of cyanogen and oxygen.

Water holding caustic potash in solution produces the same changes upon cyanogen; but much more rapidly than pure water, and the new acids evolved, instead of combining with ammonia, unite with the potash itself, while the ammonia is disengaged.

When water impregnated with cyanogen is digested over peroxide of mercury, its peculiar odour soon disappears, and a portion of the oxide is dissolved. When the solution is concentrated in a retort, carbonate of ammonia passes into the receiver, and two sets of crystals are deposited. The first set consists of cyanide of mercury. The other crystals differ in their shape; but whether they consist of cyanic acid and oxide of mercury could not be ascertained. Hydrocyanic acid is also present in the liquid.

Hydrocyanic acid when placed in contact with the perhydrate of copper unites with it, and forms a yellowish-green compound, which crystallizes in small grains, and when washed in boiling water becomes white. The red matter which is formed by dropping common prussiate of potash into a solution of copper is in Vauquelin's opinion a hydrate. It acquires a green colour when treated with liquid ammonia. This he considers as its true colour when deprived of water.

From the observations contained in this paper, it would appear that Vauquelin has no knowledge of the ferro-chyazic acid of Porrett. Many of his conclusions are erroneous, from his not having attended to the difference between the hydrocyanic and the ferro-chyazic, two very distinct substances, which he seems to me to have always confounded together. Thus one of the most prominent parts of the paper consists in a set of experiments to ascertain whether prussian blue be a cyanide or a hydrocyanate of iron; he concludes from them that it is a hydrocyanate. It does not seem to have occurred to him that it may likewise be a ferrocyanate of iron, which indeed is the most likely opinion of all.

Finally, M. Vauquelin has shown by his experiments that when the cyanide of potash comes in contact with water, there is always formed a quantity of carbonate of ammonia. This fact deserves the attention of the manufacturers of prussian blue.—(Ann. de Chim. et Phys. ix. 113.)

III. ACIDIFIABLE COMBUSTIBLES.

The acidifiable combustibles have been recently enriched with a new substance, I mean *selenium*—a substance detected by Berzelius, and approaching nearest to sulphur in its properties, though it differs in many respects from this combustible. I gave a sketch of its characters in the History of the Chemical Sciences for the preceding year. But the publication of Berzelius's experiments on it which has since taken place, will enable me to add several important particulars with which at that time we were unacquainted. Some few additions have been made to our knowledge of hydrogen, phosphorus, and carbon, which I shall notice under this division, to which these bodies belong.

1. *Hydrogen.*—My readers are aware that some years ago Dr. Prout showed from the specific gravity of ammoniacal gas that the specific gravity of pure hydrogen gas was less than had hitherto been supposed, and that in reality, instead of being the 15th part of the weight of oxygen gas, it was only the 16th part of that weight. From the celebrity of the chemists both in this country and in France, who had undertaken to determine the specific gravity of this gas, and who had concluded it to be to oxygen gas as 1 to 15, it is not at all surprising that this correction of Dr. Prout made very little impression upon the chemical world. It would have been wonderful indeed if that had not been the case, and if some of those chemical understrappers who are unable to think with precision, or indeed to think for themselves at all, had not come forward with their sneers, as if it were an unpardonable crime to deviate in any respect from the *ipse dixit* of those individuals whom they have thought proper to set up as the gods of their idolatry. All this was to be expected, and it took place accordingly. My readers will see, by a notice in the *Annals of Philosophy*, xiii. 317, that Berzelius and Dulong have lately made a new set of experiments on the specific gravity of hydrogen gas. They have found it lighter than preceding experimenters, or very nearly 0·069, which is precisely the specific gravity deduced by Dr. Prout from other considerations. This result has been verified in my laboratory. We found the specific gravity of hydrogen gas in three trials 0·06933.

The result of the curious attempts of Thenard to add indefinite quantities of oxygen to the acids by means of peroxide of barytes, of which an account will be found in the *Annals of Philosophy*, xiii. 1, is the discovery of a deutoxide of hydrogen, or a compound of oxygen and hydrogen, containing twice as much oxygen as water does. This deutoxide is a fluid less volatile than water, and may, therefore, be nearly freed from that liquid by spontaneous evaporation in an exhausted receiver containing sulphuric acid.

This deutoxide has the property of whitening all vegetable bodies. Probably, therefore, it is formed during the action of chlorine on cloth in the modern process of bleaching. I conceive a portion of the water to be decomposed, so that its hydrogen converts the chlorine into muriatic acid, while its oxygen converts another portion of the water into deutoxide of hydrogen. The art of bleaching then will have reached perfection when a cheap process is discovered of making deutoxide of hydrogen on a large scale.

2. *Carburetted Hydrogen Gas.*—Mr. Faraday has pointed out what he considers as a mistake in the generally received opinions respecting carburetted hydrogen gas. It is generally believed, he says, that chlorine has no action on this gas, whereas he finds

that the two gases when mixed act upon each other very readily. They explode when exposed to the direct rays of the sun even in winter, and speedily act upon each other even in the light of day. A portion of the substance formed by the union of chlorine and olefiant gas, to which I have given the name of *chloric ether,* is formed. Muriatic acid is also formed. Mr. Faraday concludes from these phenomena that this substance is not a compound of chlorine and olefiant gas, but of the elements of these gases arranged in another form.

The action of chlorine on carburetted hydrogen has been much longer known than Mr. Faraday seems to have been aware of. Unless my memory deceives me, it was pointed out by Mr. Cruickshanks in a paper on heavy inflammable airs, published in the fourth volume of Nicholson's quarto journal. He was not aware that mixtures of these two gases explode; but he showed that in 24 hours they destroy each other's elasticity completely. —(Institution Journal, vi. 358.)

As to the compound formed by the union of chlorine and olefiant gas, I cannot admit the accuracy of Mr. Faraday's notion respecting it; for I find that when equal volumes of chlorine and olefiant gas are mixed together, they are totally condensed into the liquid compound. No muriatic acid is formed, or at least none retains the elastic state. The chlorine may be again separated from the olefiant gas, as I showed long ago in my paper on the carburetted hydrogen gases.

3. *Hydrocarbonic Gas.*—I had the good fortune to discover this gas last year, during a set of experiments on prussiate of potash. It is easily obtained by heating in a retort a mixture of prussiate of potash and sulphuric acid. It is colourless, not sensibly absorbed by water, has a peculiar smell, a somewhat aromatic taste, and it leaves a hot impression in the mouth. Its specific gravity is 0·993. It is combustible, and burns readily with a deep blue flame. Three volumes of it require for complete combustion two volumes of oxygen gas. The residue after combustion amounts to three volumes, and is carbonic acid gas. Hence it is obviously a compound of

$$\left.\begin{array}{l}\text{3 volumes carbonic oxide}\\ \text{1 volume hydrogen gas}\end{array}\right\}\text{condensed into three volumes.}$$

—(See *Annals of Philosophy,* xii. 104.)

M. Gay-Lussac mentions in a note upon an extract from my paper, which he has done me the honour to insert in the Annales de Chimie et Physique, that Berthollet has already distinguished those gases which are analogous to mine in their composition by the name of *oxycarburetted hydrogen.* But a very little consideration will, I am sure, satisfy this very ingenious chemist, that such a name could not, with any attention to propriety, be given to the gas which I have here described. It is obviously much nearer carbonic oxide in its properties than it is to carburetted hydrogen. Indeed I conceive it to be merely carbonic oxide united to one third part of its volume of hydrogen gas. The name ought to indicate this, which I think is done by calling it *hydro-carbonic oxide* gas.

4. *Phosphorus.*—Sir Humphry Davy's experiments on the combustion of phosphorus, published in the Philosophical Transactions for 1818, and of which an account was given in the *Annals of Philosophy*, xiii. 210, exactly tally with my deductions from a set of experiments on phosphuretted hydrogen, and show in the most decisive manner, if any doubts were entertained on the subject, that the previous experiments of that gentleman on the combustion of phosphorus were inaccurate. I think it established by these experiments of Davy and by my own that the weight of an atom of phosphorus is 1·5, and that the composition of the two acids of phosphorus is as follows:

	Phosphorus.	Oxygen.
Phosphorous acid...............	1·5 +	1
Phosphoric acid	1·5 +	2

But it must not be concealed that these numbers will not agree with the equivalent number for phosphoric acid as deduced from Berzelius's analysis of the different phosphates. I think, therefore, that the subject still claims further investigation.

5. *Selenium.*—In the historical sketch of the progress of chemistry during the year 1817, inserted in the *Annals of Philosophy*, xii. 1, will be found (p. 13) a short account of the properties of this new substance. The kindness of Prof. Berzelius has put it in my power to examine this substance myself, and to make a few trials on its more prominent properties. And the full account of its properties has been given to the public by that skilful chemist in the fifth volume of the Afhandlingar; and a translation of his paper has been inserted in the ninth volume of the Annales de Chimie et Physique, from which I have extracted it for the sake of the readers of the *Annals of Philosophy*. From this paper, which will not have escaped the recollection of the reader, I shall select those circumstances of importance which were omitted last year.

Berzelius made an ingenious set of experiments to determine the composition of selenic acid, and to deduce from this composition the weight of an atom of selenium. He saturated a given weight of selenium with chlorine, and formed what he considered as a double acid; but which was obviously a chloride of selenium. When this chloride is treated with water, it is converted, as is the case with other chlorides, into selenic acid and muriatic acid. This is obviously occasioned by the decomposition of water, the hydrogen of which unites with the chlorine, and converts it into muriatic acid; while the oxygen unites to the selenium, and converts it into selenic acid. Hence if we know the weight of the selenium employed and of the muriatic acid produced, we shall have it in our power to determine the quantity of oxygen which united with the selenium and served to acidify it. One part of selenium, when converted into a chloride, was found to weigh 2·79 parts. This chloride being dissolved in water, and precipitated by nitrate of silver, the precipitate was washed with boiling water, acidulated with nitric acid, till all the seleniate of silver at first precipitated was redissolved. The residual chloride of silver was dried and fused. It weighed 7·2285. Now chloride of silver is a compound of one atom silver = 13·75 + 1 atom chlorine = 4·5. Of course the quantity of chlorine in 7·2285 of chloride of silver must be 1·782. To convert this chlorine into muriatic acid, we must combine it with 0·0495 of hydrogen. Now as the weight of the oxygen in water is eight times as great as that of the hydrogen, it is evident that if we multiply 0·0495 by 8, we shall have the weight of oxygen that is requisite to convert one of selenium into selenic acid. But 0·0495 × 8 = 0·38. Hence it follows, if the experiment was accurately made, that selenic acid is a compound of

Selenium	100
Oxygen	38
	138

The constituents of the acid, as deduced by Berzelius from the preceding experiment, are,

Selenium	100·00
Oxygen	40·33

His mode of reasoning was quite different from that which I have followed; but the difference in our results is owing to small differences in our notions respecting the constitution of chloride of silver. I see no reason for doubting that the weight which I have assigned to an atom of silver; namely, 13·75, is the true weight. Berzelius's number is equivalent to 13·44. Hence the quantity of chlorine in chloride of silver is a little more than I make it; and from this arises the difference in the weight of our oxygen.

If we suppose the chloride of selenium formed by Berzelius to be a compound of two atoms of chlorine and one atom of selenium, as is most likely, then we have 1·79 : 1 :: 2·5 : 2·514, and 2·514 × 2 = 5·028 will be the weight of an atom of selenium. The weight of an atom of selenium derived from the notion that selenic acid is a compound of one atom selenium and two atoms oxygen will be 5·263; but the first estimate is probably most accurate. In the present state of our knowledge, we might reckon 5 as the weight of an atom of selenium.

On this supposition, which cannot be very far from the truth, an atom of selenic acid will weigh 7. Now this corresponds pretty well with the constitution of the only two seleniates of which Berzelius has given us the analysis. Seleniate of barytes he found composed of

Selenic acid.......	100·0	7·00
Barytes	137·7	9·75

The constituents of seleniate of soda, according to his analysis, were,

Selenic acid.......	64·5	7·267
Soda	35·5	4·000

The equivalent for selenic acid derived from the first of these salts is 7, which corresponds with the weight of an atom of selenic acid derived from the choride of selenium. The equivalent number derived from the second salt is 7·267, which corresponds with the weight of an atom of selenium derived from the quantity of oxygen indicated by the chlorine with which it had been in combination. As we have no means of determining which of these two results is the most accurate, the proper mode of proceeding, in the present state of our knowledge, seems to be to take the mean of the two as the true number. On that supposition 5·125 will be the weight of an atom of selenium, and 7·125 the weight of an atom of selenic acid.

Selenium then approaches arsenic in the weight of its atom. It constitutes another exception to the law which Œrsted has endeavoured to establish, that acidifiable bases always combine with a great quantity of oxygen compared to their own weight; while alkalifiable bases unite with a small quantity. Indeed nothing can be more hazardous than the establishment of general laws in chemistry from the very imperfect inductions which the present very limited knowledge which we possess enables us to make. In a few years, the discovery of some new substance which spurns our laws, is sure to overturn all our fine constructed fabric, and to give us a mortifying proof of how very inadequate judges we are of the general laws by which the constitution of the world is maintained.

Selenium, like sulphur, phosphorus, and carbon, has the property of uniting with hydrogen, and forming *selenuretted hydrogen gas*. Berzelius obtained this gas by fusing together potassium and selenium, and treating the selenuret with diluted muriatic acid. This gas has the smell of sulphuretted hydrogen. It acts with great violence upon the throat and fauces, producing symptoms of rather an alarming nature. It is more soluble in water than sulphuretted hydrogen gas. The aqueous solution precipitates all the metals, and the colour of the precipitates is black or brown, except those of manganese, zinc, and cerium, which are flesh-coloured. The black and brown precipitates are selenurets, the red are hydroselenurets. Selenuretted hydrogen gas is readily decomposed by the concurrent action of water and air. There is reason to conclude, from the analysis of this gas by

Berzelius, that it is a compound of one atom selenium and one atom hydrogen, or by weight of

Selenium	5·125	41
Hydrogen	0·125	1

These would be the weights nearly, if we were to go over Berzelius's experiments and modify the results in order to make them correspond with the weight of an atom of silver, oxygen, and hydrogen, as I have established these weights in a preceding volume of the *Annals of Philosophy.*

Selenium combines with sulphur, phosphorus, and with all the metals tried. The phenomena, which take place during the formation of these selenurets are analogous to those which are exhibited by sulphur when it unites with the metals. For a particular account of the few facts ascertained by Berzelius respecting these selenurets, I refer to the paper of that indefatigable chemist in the last number of the *Annals of Philosophy.*

Selenium, like sulphur, combines likewise with ammonia, with the fixed alkalies, and the alkaline earths. There is a striking analogy between the selenurets and sulphurets of these bases.

6. *Protoxide of Azote.*—I may notice here that Mr. Faraday has pointed out the reason why the respiration of this gas sometimes produces alarming effects upon the health of the person who employs it (Institution Journal, vi. 360). These effects are owing to the mixture of sal ammoniac with the nitrate of ammonia, from which the gas was procured. When such an impure salt is employed, the sal ammoniac is decomposed in the first place, and there are evolved azotic gas, chlorine, &c. which are mixed with the protoxide of azote, and occasion the injurious effects.

I must observe that these facts were pointed out many years ago in a paper published by Proust. He ascertained that the presence of sal ammoniac injured the protoxide of azote. He assures us that the first portions of gas driven off from such a mixed salt are of a peculiar nature. This assertion has not hitherto been verified, nor so far as I know even examined.

7. *Sulphuretted Azote.*—It seems hardly necessary to notice the statement of Dr. Granville, that a gas composed of sulphur and azote is sometimes found in the abdomen in peculiar circumstances. I have related in a former historical sketch, given in a preceding volume of the *Annals of Philosophy,* the discussions respecting this supposed compound which took place in Germany, and the unsuccessful experiments of Berzelius and others, who endeavoured to form it, or to obtain some evidence of its existence. A compound of 10½ sulphur + 89½ azote would consist of one atom of sulphur united to about 9·7 atoms azote—a very unlikely compound indeed, and unlike that of any gas containing hydrogen with which we are acquainted. I have no doubt that what Dr. Granville took for sulphuretted azote was a mixture of

azotic gas and sulphuretted hydrogen. Such a mixture would not burn, and nothing was easier than to overlook the hydrogen in the analysis.

IV. ALKALIFIABLE COMBUSTIBLES.

This department has been recently enriched by the discovery of three new metals in Germany. Of these, the first, cadmium, was noticed in the historical sketch of last year. Since that period I have had an opportunity of examining the metal myself, and of verifying the accuracy of the account of it which has been published by Stromeyer. The other two metals, vestium and wodanium, were unknown, or at least the knowledge of them had not reached me when I drew up my historical sketch last summer. Vestium was discovered by Professor West, who has not yet succeeded in obtaining it in a state of purity; but if his experiments be accurate, it is undoubtedly a substance which possesses distinct properties from every metal known, and of course it is entitled to claim the rank of a new and peculiar metal. Wodanium has been announced by Lampadius as discovered in a mineral specimen found in the cabinet of Von Trebra, and which had been obtained from Hungary. We must suspend our judgment respecting it till Lampadius has published his experiments, and till he has put it in the power of some other skilful chemist to repeat them.

1. *Cadmium.*--The discovery of this metal was made by Stromeyer. He was inspecting the apothecaries' shops in the principality of Hildesheim, and found that the carbonate of zinc was substituted in that country for the oxide of zinc, the use of which had been ordered in the pharmacopœia. This carbonate of zinc was manufactured at Salzgitter. Upon inquiry, he learned from Mr. Jost, who managed that manufactory, that they had been obliged to substitute the carbonate for the oxide of zinc, because the oxide had a yellow colour, and was in consequence unsaleable. On examining this oxide, Stromeyer found that it contained a small proportion of the oxide of a new metal, which he separated and reduced, and to which he gave the name of cadmium.

Cadmium is white, like platinum. It is hard, has a hackly fracture, is malleable and ductile, and has a specific gravity of 8·750 after fusion. It melts below a red heat, and is likewise very volatile, rising in the state of vapour at a temperature not much higher than the boiling point of mercury.

It unites with only one proportion of oxygen, and forms an oxide of a greenish-yellow colour, fixed in the fire, and infusible at a white heat; but assuming a yellow, or even brown colour. This oxide may be formed by heating the metal in the open air, it catches fire, and sublimes in a yellow smoke, which is the oxide.

It dissolves in nitric acid with the evolution of nitrous gas, and in sulphuric and muriatic acids with the evolution of hydrogen gas; but the solutions in these last two acids go on very slowly. All the acid solutions of cadmium are colourless; and the salts which it forms with acids are white.

The sulphate, nitrate, muriate, and acetate of cadmium, crystallize readily, and are very soluble. The phosphate, carbonate, and oxalate of cadmium are insoluble.

The oxide is thrown down white by the fixed alkalies, probably in the state of a hydrate. Ammonia, and likewise its carbonate, redissolves the precipitated oxide. Hence it is easy, by means of carbonate of ammonia, to separate cadmium from zinc or copper when they happen to be mixed.

Prussiate of potash throws it down *white;* sulphuretted hydrogen, or a hydrosulphuret, throws it down *yellow.*

Zinc precipitates cadmium from its acid solutions in the metallic state; but cadmium throws down copper, lead, silver, and gold, in the metallic state. It belongs, therefore, to my fourth family of alkalifiable combustibles, and must be placed immediately after zinc.—(See *Annals of Philosophy,* xiii. 108.)

2. *Vestium.*—The characters of this metal have been so imperfectly ascertained, and Mr. West's paper on the subject has appeared so lately in the *Annals of Philosophy* that it will be sufficient, I conceive, to refer those readers who are curious on the subject to the paper itself.

3. *Wodanium.*—The few characters of this metal which have been communicated to the public by Lampadius (see *Annals of Philosophy,* xiii. 232) are sufficient to satisfy us that it is entitled to be considered as a peculiar metal.

Its colour is bronze yellow; it is malleable; has a hackly fracture; the hardness of fluor spar; and is strongly attracted by the magnet. Its specific gravity is 11·470.

Its oxide is black, and is easily formed by heating it in contact with the air.

It dissolves in acids, and the solutions have a light wine-yellow tinge. The alkaline carbonates throw it down white; caustic ammonia precipitates it blue.

Nitric acid dissolves both the metal and the oxide with facility, and the solution yields white needle-form crystals, which dissolve readily in water.

A plate of zinc throws it down black. Prussiate of potash throws it down pearl grey.

4. *Cyadide of Potassium.*—When potash is calcined with an animal substance, the compound formed is not a cyadide of potash, but a cyadide of potassium; for if cyanogen be united directly with potassium and the compound be dissolved in water, it is converted into hydrocyanate of potash. Acids decompose it, hydrocyanic acid is disengaged, and no ammonia is formed; but if cyanogen be absorbed by a solution of potash, and an acid be added to the solution, there are disengaged carbonic acid,

hydrocyanic acid, and ammonia, each amounting to the volume of the cyanogen absorbed. The first two of these bodies are disengaged immediately on the addition of the acid; but the ammonia does not make its appearance till there be added an excess of lime. Now when the product of the calcination of an animal substance with potash is dissolved in cold water, and then treated with muriatic acid, and finally, with lime, it exhibits the same phenomena as the cyadide of potassium. These important facts have been ascertained by M. Gay-Lussac.

It is important not to throw potash calcined with an animal substance into water, while red-hot, or even hot; for in that case it is decomposed, and a great quantity of ammonia is produced. If it be allowed to cool in the open air, it is apt to catch fire, and burn like a pyrophorus.—(Ann. de Chim. et de Phys. viii. 440.)

6. *Action of Iron on Water.*—It has been generally admitted by chemists that iron is capable of decomposing water at the ordinary temperature of the atmosphere; though I am not aware that any set of experiments establishing this fact has been published, except those of Lavoisier, which, however, are perfectly decisive. This gentleman mixed together iron filings, and distilled water, freed from its air by boiling, and placed them under a receiver filled with mercury at the ordinary temperature of the atmosphere. Hydrogen gas was evolved in abundance.—(See Mem. de l'Acad. des Sciences, 1781, p. 478.)

Dr. Marshall Hall, who does not appear to have been aware of these experiments of Lavoisier, nor with the experiments of M. Guibourt on the same subject, published in the Journal de Pharmacie, for June, 1818 (p. 241), has inserted a paper in the last April number of the Journal of the Royal Institution, or of the Quarterly Journal, as it has now denominated itself, the object of which is to prove that iron has not the property of acting on water deprived of air; and that in all cases where it was supposed to have been oxidized under water, the change was merely the consequence of the action of atmospherical air. I have no doubt that Dr. Hall made his experiments with sufficient care and precision; yet I think them insufficient to decide the question. He put a small quantity of iron into a great quantity of water. Now I happen to have made similar experiments to his many years ago. I found that in such cases no sensible quantity of hydrogen was extricated after an interval of several weeks; but if the mixture was kept for some hours at the boiling temperature, I always obtained a sufficient quantity of hydrogen to ascertain its nature. M. Guibourt, in his paper above alluded to, has gone much further, and indeed placed the subject in a very clear point of view. When a small quantity of water is mixed with a great quantity of iron, the decomposition of that liquid goes on rapidly; but when a great quantity of water is mixed with a small quantity of iron, no sensible decomposition takes place unless the temperature be considerably elevated.

7. *Softening and tempering Steel.*—Mr. Gill informs us, in the *Annals of Philosophy*, xii. 58, that if steel be heated below the hardening point, and then plunged into cold water, it will be softened thereby, and in a much superior manner to the common process. He says, likewise, that if steel be heated to the requisite degree, and plunged into a bath composed of a mixture of lead and tin, similar to plumber's solder, heated to the requisite temperature, it will be at once tempered and hardened. It is for artists to determine whether these methods will answer, and whether they be preferable to the common ones.

8. *Manganese.*—It is now about 44 years since this metal was reduced by Assessor Gahn, of Fahlun; yet I am not aware of any additional facts respecting it since that period, except those contained in Dr. John's elaborate paper on this metal published in 1807. M. Fischer, of Schaffhausen, a manufacturer of cast steel, having discovered the means of producing a very intense heat in his furnaces, has been enabled in consequence to reduce this very refractory metal to the metallic state. The following are the characters of metallic manganese, as described by the editors of the Bibliotheque Universelle, to whom M. Fischer sent a specimen of the reduced metal.

Its colour is whitish; it is harder than tempered steel; it cuts glass nearly as well as the diamond; it scratches rock crystal. It acquires a very good polish, which is probably not durable, in consequence of its great affinity for oxygen. When kept under water for 24 hours, it becomes covered with a coat of brown oxide. It sensibly attracts the magnetic needle; but was not, perhaps, quite free from iron. Its specific gravity is 7·467. This is considerably under the estimate of Dr. John, who found it 8·013. This I consider as an additional proof of the impurity of the manganese of M. Fischer.—(Biblioth. Universelle, vi. 232.)

In the fifth edition of my System of Chemistry (vol. i. p. 403), I have endeavoured to show that manganese forms only two oxides, the green, and the black. The composition of these oxides, I consider to be as follows:

Protoxide........	100 manganese + 28·75 oxygen
Peroxide	100 manganese + 57·50 oxygen

On this supposition an atom of manganese weighs 3·5. Berzelius had long before made a set of experiments on these oxides, and had determined their composition to be as follows:

Protoxide...........	100 metal + 28·107
Peroxide............	100 metal + 56·214

M. Arvedson has lately repeated the experiments of Berzelius, and obtained the same results. This certainly gives considerably additional weight to the determination of Berzelius, whose well-known precision entitles all his experiments to the greatest attention. My numbers were pitched upon from theoretical considerations. I am not disposed to change them till it has been shown in a satisfactory manner that they are inconsistent with experiment. The proportion of oxygen which I have given differs only about $\frac{1}{48}$th part from that given by Berzelius. Now I am very much afraid that the limits of unavoidable error in such experiments are greater than $\frac{1}{48}$th of the whole. Hence we have no means of coming at the truth except by theoretic views, which will guide us to new experiments; and when these are sufficiently multiplied, we shall obtain a mean approaching very near the truth.

M. Arvedson, during his experiments, made a discovery of rather an interesting nature, and deserving the attention of the manufacturers of the bleaching salt and bleaching liquor. He found that there are two native black oxides of manganese. The first, the common peroxide; the second, the hydrated black oxide, which he found composed as follows:

Oxidum manganoso-manganicum	89·92
Water	10·08
	100·00

This oxidum manganoso-manganicum is a compound of two atoms of peroxide and one atom of protoxide of manganese; or it contains $\frac{1}{6}$th less oxygen than the peroxide. If the oxygen in the water be added to that of the oxidum manganoso-manganicum, the whole will be converted into peroxide of manganese.—(Jour. de Phys. lxxxvii. 464.)

There is reason to believe from the late experiments of Chevillot and Edwards, that manganese is capable of combining with an additional atom of oxygen, and of forming a new compound, which seems to possess acid properties, and to act with great energy on combustibles. They have not yet given us the proportions of manganese and oxygen which exist in this compound; but they have shown that red chameleon mineral is a compound of potash, black oxide of manganese, and oxygen, which are all present in definite proportions; that the quantity of oxygen depends upon that of the manganese present, and not upon that of the potash; that the combination is neutral, and possesses the characters of a salt; and that when an excess of potash is added, the chameleon assumes a green colour. When these crystals are heated in contact with hydrogen gas, they set it on fire. They detonate violently with phosphorus, set fire to sulphur, arsenic, and antimony, and indeed to all combustible bodies hitherto tried. Were we to suppose this manganesic acid (as Chevillot and Edwards have termed it) a compound of one atom manganese and three atoms oxygen, its constitution would be as follows:

Manganese	3·5	100·00
Oxygen	3·0	85·71

These facts claim the careful examination of chemists. If they be verified, they will exhibit the remarkable and hitherto unique example of the same base forming a perfect salifiable base and a perfect acid simply by uniting with different proportions of oxygen. This would be a fine confirmation of the theory advanced by Œrsted respecting the cause of acidity and alkalinity, of which an account has been given in a late number of the *Annals of Philosophy.*—(See Ann. de Chim. et Phys. viii. 337.)

9. *Cobalt and Nickel.*—The most difficult problem, perhaps, in practical chemistry is the separation of these two metals from each other. A variety of methods have been proposed, all of which I have tried, with some additional ones of my own, without having yet hit upon one which is not either imperfect, or at least liable to some very serious objection. When into a concentrated solution of cobalt in sulphuric or muriatic acid, a solution of tartrate of potash is added, a triple salt is formed, consisting of tartaric acid, united at once with potash and with oxide of cobalt, which crystallizes in large flat rhomboidal prisms. These crystals, so far as I have examined them, contain no other metal except cobalt; but this method, though promising at first sight, I did not find to answer so well as I expected; for the tartrate of potash undergoes spontaneous decomposition when the solution is left to spontaneous evaporation; and if the evaporation is produced by the action of heat, the crystals formed are ill defined, and consequently liable to be impure.

It was with great pleasure, therefore, that I perused a paper by M. Laugier, published in the Annales de Chimie et Physique for November, 1818, on the mode of analyzing the ores of cobalt and nickel, and on the best method of separating these two metals from each other. After trying every known method of separating these two metals from each other without succeeding, MM. Laugier and Silveira were on the point of abandoning the investigation, when it occurred to them to try the effect of a concentrated solution of ammonia on the impure oxalate of nickel. A solution took place of a fine azure colour. On exposing this solution to the open air, the ammonia gradually made its escape, and at the same time the oxalate of nickel precipitated to the bottom of the vessel; while the whole of the oxalate of cobalt remained in solution. Thus it is easy to separate these two metals from each other by converting them into oxalates, treating the oxalates with ammonia, and leaving the ammoniacal solution for some days in an open vessel. I applied this method as a test to ascertain the purity of the nickel and the cobalt which I had purified before M. Laugier's paper came into my possession. I had the satisfaction to find that it neither indicated the presence of nickel in my cobalt, nor of cobalt in my nickel; therefore, if M. Laugier's method be a good one, I had succeeded beforehand in accomplishing a complete separation of these two metals.

The method of proceeding to analyze the ores of cobalt suggested to Laugier by the preceding facts, is the following:

(1.) Let the ore be roasted to drive off as much of the arsenic as possible.

(2.) Dissolve the roasted ore in nitric acid and evaporate nearly to dryness to get rid of the arsenious acid.

(3.) Pass a current of sulphuretted hydrogen gas through the liquid till the whole of the arsenic and copper (if any be present) be thrown down.

(4.) Heat the liquid to drive off the excess of sulphuretted hydrogen, and precipitate the metals by means of carbonate of soda.

(5.) Treat the carbonates with oxalic acid to separate the iron. Then dissolve the oxalates of cobalt and nickel in ammonia to separate these two metals.

Laugier informs us that he detected nickel in the cobalt ore of Tunaberg, though the presence of that metal had not hitherto been suspected in it.

10. *Brass.*—I was much amused by a remark which Mr. Gill has thought proper to make upon an observation of mine in my Historical Sketch of Chemical Science for 1817. I stated the well-known fact that old Dutch brass was much more valued by watchmakers than British brass, and gave my reasons for the difference between them. The Dutch brass is a compound of two atoms copper and one atom zinc; while English brass is a compound of one atom copper and one atom zinc. I think I generally write so perspicuously that my meaning can hardly be mistaken; yet Mr. Gill insinuates, in pretty broad terms, that I considered the partiality of watchmakers for the Dutch brass as a prejudice (*Annals of Philosophy*, xii. 125); though I had stated, in as clear a manner as I could, the reason of the superiority of the Dutch over the British. The prejudice of my friend, the watchmaker, did not consist in considering the Dutch brass as better for his purpose than the English, which is really the case; but in supposing that the art of making that good kind of brass is lost. I pointed out how it might be easily manufactured at the pleasure of the brass maker; and Mr. Gill informs us in the article already quoted that his father in-law intended to set up a manufactory of this old superior kind of brass. I am glad to hear it. He will prove the truth of what I ventured to assert on general grounds, that modern brass makers may, if they think proper, make as good brass as that which the watchmakers value so highly.

Whatever Mr. Gill may think upon the subject, I must be allowed to consider my observations as of some importance. They were founded on experiment, and they explained a fact generally known, but not previously accounted for, that old Dutch brass is superior in ductility, &c. to English brass.

11. *Bismuth.*—It was observed many years ago by Dufoy

that bismuth may be substituted for lead in the process of purifying gold and silver by cupellation; but no accurate experiments had been made to determine whether that metal could be employed to ascertain exactly the quantity of alloy contained in gold and silver. We are indebted to M. Chaudet for solving this problem. He has ascertained that the bismuth to be employed in such cases must be free from silver; that if it contains arsenic, which is commonly the case, a portion of the silver is driven off the cupel along with the arsenic and lost: that a smaller proportion of bismuth must be employed than is required of lead; and that the cupels employed must be less porous than those used when lead is used to separate the alloys from gold or silver; because bismuth has the property of inducing so great a degree of fluidity into those metals that they are apt to penetrate into the pores of an ordinary cupel and to be lost. It follows from the experiments of Chaudet, that if these precautions be attended to, bismuth may be employed as well as lead to determine the purity of gold and silver. The following table exhibits the quantity of bismuth requisite for purifying one part of silver of the degrees of purity marked in the table:

Alloy.		Dose of bismuth necessary.	Ratio between the bismuth and copper.
Silver.	Copper.		
1000	0	$\frac{1}{10}$	0·0
950	50	2	40·0
900	100	3	30·0
800	200	6	30·0
700	300	8	26·6
600	400	10	25·0
500	500	11	22·0
400	600	12	20·0
300	700	12	17·0
200	800	12	15·0
100	900	12	13·3
0	1000	8	8·0

(Ann. de Chim. et Phys. viii. 113.)

12. *Tin.*—This metal has so great a tendency to unite with a maximum of oxygen that the preparation of its protoxide is attended with some difficulty. I have generally succeeded by keeping the permuriate of tin in a close vessel in contact with a quantity of metallic tin, and then precipitating the protomuriate by ammonia; but this method is not always attended with the desired success. M. Cassola has given us a process which he assures us never fails. Upon filings of tin, he pours nitric acid diluted with ten times its volume of water, and leaves the two substances in contact for 48 hours. The tin acquires a brownish-

black colour, and is completely converted into protoxide. The nitric acid contains in solution a portion of protoxide. When kept for some time, it lets fall an insoluble subnitrate, which is gradually changed into peroxide of tin. Besides the protoxide, there is a yellowish light matter which floats about in the liquid, and which may be separated by the filter. It is a protohydrate of tin. Acetic acid, when left in contact with tin filings, dissolves a portion, and converts it into protacetate of tin, but the residual tin filings are not oxidized.*—(See Giornale di Fisica, Chimica, &c. 1818, p. 378.) The observations which M. Cassola makes on the peroxide of tin contain nothing which has not been long known to chemists. This peroxide is not white, as is stated in some recent systems of chemistry, but yellow; and it is insoluble in all the acids which I have tried. Its hydrates (for there are several) are of a fine white colour, and dissolve readily in muriatic, but not in nitric acid.

13. *Mercury.*—M. de Blainville has made an observation which seems entitled to attention, and which, therefore, I notice here. It is well known to chemists that mercury amalgamates very easily with gold, silver, lead, tin, zinc, bismuth, and arsenic; but it does not amalgamate with iron, cobalt, and nickel; or at least the amalgams of these metals cannot be formed without considerable difficulty. Now the observation of M. de Blainville is, that when these metals are united to arsenic, the alloy amalgamates very readily; so that by the intervention of this metal, we can easily procure amalgams of those metals which do not, in other circumstances, unite to mercury.—(Jour. de Phys. lxxxiv. 267.)

I was rather surprised to find (*Annals of Philosophy*, xii. 67) that Mr. Donovan had concluded from his experiments that the composition of the oxides of mercury is as follows:

Protoxide..... 100 mercury + 4·12 oxygen
Peroxide. .... 100 mercury + 7·82 oxygen

These numbers are inconsistent with the doctrine of definite proportions, which has been perfectly well established. The experiments of Fourcroy, Sefstrom, &c. have shown that the composition of these oxides is as follows:

Protoxide. ...... 100 mercury + 4 oxygen
Peroxide .... 100 mercury + 8 oxygen

14. *Copper.*—It seems hardly worth while to recall the attention

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* I may observe here that I had the curiosity to try this process, but did not find it to answer. The mode which I have usually followed to obtain protoxide of tin is to dissolve that metal by means of heat in muriatic acid. I put the solution into a well-stopped phial, placing in it a number of slips of tin. These slips gradually reduce the whole of the dissolved metal to the state of protoxide. I have sometimes seen the dissolved tin precipitated upon the tin in crystalline plates, having the metallic lustre.

of chemists to the specific gravity of the best Japan copper, which I found only 8·434. It is merely valuable, because it enables us to correct the statements of Cronstedt and Bergman upon this subject. They made the specific gravity of Japan copper above 9. Now the specific gravity of the very purest copper which can be procured is somewhat under 9, even when it has been hammered or passed between rollers.—(*Annals of Philosophy*, xiii. 224.)

I may allude also to the specimen from the mint which I analyzed, and which I found a mixture of protoxide of copper, oxide of iron, and sand. It is valuable merely, because it shows that the protoxide of copper may, in certain circumstances, be formed by the application of heat to metallic copper.

M. Chaudet, to whom we owe a set of experiments on the possibility of separating tin from antimony and bismuth by means of muriatic acid, experiments of which an account has been given in a preceding historical sketch, has more recently made a set of experiments to determine whether the same acid be capable of separating tin from copper when the two metals are alloyed together. The result was, that in whatever proportion the two metals are alloyed, they cannot be accurately separated by muriatic acid. In general the copper prevents the tin from dissolving so readily in muriatic acid as it would have done had it been pure. Hence the last portions consist of copper still alloyed with a notable proportion of tin.—(See Ann. de Chim. et Phys. vii. 274.)

13. *Silver.*—In the *Annals of Philosophy*, xii. 143, is given a very easy method of reducing silver from its chloride to the metallic state, for which we are indebted to M. Arvedson. The method is this: put into a conical glass a quantity of granular zinc, cover it with chloride of silver, and then pour it on some diluted sulphuric acid. The hydrogen gas evolved speedily reduces the silver to the metallic state. I have verified this method, and found it to answer perfectly well.

14. *Platinum.*—Mr. Heuland has favoured us with an authentic account of the mass of platinum deposited in the Royal Museum at Madrid. It is obviously the largest mass hitherto found. It weighs above a pound and three quarters.—(*Annals of Philosophy*, xii. 200.)

I beg leave to call the attention of those manufacturers who have occasion for platinum vessels to the mode of purifying that metal proposed by the Marquis of Ridolphi, of which an account will be found in the *Annals of Philosophy*, xiii. 70. It will not yield a pure metal, but I think it likely that it would answer sufficiently for all the purposes to which that metal is applied by manufacturers, and it would enable them to procure the metal at a much smaller price than can be at present charged for it, in consequence of the very expensive process by which it purified.

If we can believe the accounts which have reached us from

Vienna, M. Prechtel, director of the Polytechnic Institute, has fused platinum in furnaces by subjecting it to a heat of 180° Wedgewood. The fusion however seems to have been rather incomplete; for if it had been complete, one cannot see how the specific gravity should have sunk $21\frac{1}{8}$ to $17\frac{2}{3}$.—(See *Annals of Philosophy*, xiii. 229.)

V. ACIDS.

In this department, as well as in several others, a considerable number of new facts have been brought to view. Several new acids, chiefly from the vegetable kingdom, have been discovered; and several bodies, formerly considered as peculiar acids, have been shown to be nothing more than varieties of other acids previously known. I shall endeavour to place these facts before the eye of the reader in as few words as possible.

1. *Combination of Oxygen with Acids.*—Thenard thought, as has been stated in the *Annals of Philosophy*, xiii. 1, that oxygen may be united with muriatic, nitrous, sulphuric, and, indeed, all the acids tried in any proportion whatever, by combining with it the peroxide of barytes, and then throwing down the barytes by means of sulphuric acid; but he has since found that the oxygen, in reality, unites not with the acids but with the water, converting it into a deutoxide of hydrogen.

2. *Hydriodic Acid.*—It would appear from a set of experiments by Houton Labillardiere, of which an account has been given in the *Annals of Philosophy*, xii. 233, that when equal volumes of hydriodic acid and bihydroguret of phosphorus, both in the gaseous state, are mixed together, the two gases are condensed into a solid matter of a white colour, and crystallize in cubes. This compound is decomposed by water and alcohol; one volume of common phosphuretted hydrogen gas, and two volumes of hydriodic acid gas, likewise condense over mercury into a white solid matter.

3. *Sulphuretted Hydrogen.*—M. Gay-Lussac has given a formula for preparing this gas, which must prove very acceptable to practical chemists. Two parts of iron filings, and one part of flowers of sulphur, are to be mixed together and put into a matrass. As much water is to be added as will convert the whole into a paste; the matrass is then to be heated, to favour the union of the sulphur and iron. This union is indicated by the disengagement of a great quantity of heat, and by the black colour which the whole mass assumes. Sulphuric acid diluted with four times its weight of water, disengages sulphuretted hydrogen gas, from this compound, with almost as much rapidity as from an alkaline hydrosulphuret. There is no advantage in preparing this substance, till it is going to be used; for it is speedily altered, and a very short time is sufficient for preparing it. Gay-Lussac is of opinion that this singular compound is a hydrosulphuret of iron. (Ann. de Chim. et Phys.

vii. 314.) I have often practised this method since Gay-Lussac pointed it out and have found it to answer very well.

4. *Hydrosulphurous Acid.*—I have given this name to a peculiar compound formed wherever three volumes of sulphuretted hydrogen gas, and two volumes of sulphurous acid gas, both dry, are mixed together over mercury; these two gases condense each other when mixed in the above proportions. The compound formed is a solid substance, which has an orange yellow colour. Its taste is at first acid, but becomes at last hot, and continues in the mouth for some time. It gives a red colour to vegetable blues, provided the least moisture be applied at the same time. It is decomposed by liquids, and does not combine with the salifiable bases while dry. It does not precipitate barytes water, except when boiled in it for some time. It requires a higher temperature for fusion than common sulphur; but it is converted into that substance if it be kept in fusion for some time.—(See *Annals of Philosophy,* xii. 441.)

5. *Acids of Tungsten and Uranium.*—Chevreul has shown that the peroxides of these two metals have the property of reddening litmus paper, and has therefore concluded that they ought to be ranked among the acids.—(See *Annals of Philosophy,* xii. 144.) I may remark that the acid nature of these bodies had been already demonstrated by much more decisive qualities than the reddening of litmus paper. Tungstic acid combines with the acidifiable bases, and forms neutral salts, some of which are crystallizable. The peroxide of uranium unites with potash, and neutralizes it. It has been found native united to lime, and in all probability has the property of neutralizing all the salifiable bases and of forming salts with them. Uranium then agrees with manganese in being capable of forming an alkaline body with one proportion of oxygen, and an acid body with another proportion; for nothing can be more completely entitled to the appellation of a salt than the compounds which the protoxide of uranium forms with sulphuric and nitric acids.

6. *Sulpho-chyazic Acid.*—Two valuable experimental papers have appeared upon this interesting acid, one by Theodore von Grotthuss, the other by Vogel. A translation of both has appeared in the *Annals of Philosophy,* xiii. 39, 89, 101. I have verified M. Vogel's formula for preparing this acid. It is merely an improvement of the method invented by Theodore von Grotthuss. In my trials I found it to answer extremely well. Being a much shorter and easier method than that contrived by Porrett; it will no doubt be employed by chemists to enable them to procure this curious, though hitherto much-neglected acid. The modified process of Grotthuss is as follows:

Mix together equal quantities of prussiate of potash and flowers of sulphur; put the mixture into a matrass, expose it to a heat sufficient to fuse it, and keep it in a state of fusion for an hour after it has ceased to give out air bubbles; then reduce the fused mass to powder, and pour hot water on it; filter the solution and drop into it caustic potash till all the iron which it contains is precipitated; filter the liquid and concentrate it sufficiently by evaporation. The sulpho-chyazic of potash is obtained in crystals. This salt is white, deliquesces in the air, and is very soluble in alcohol. When a solution of this salt is dissolved in water and mixed with sulphuric acid, it yields, when distilled, water, holding pure sulpho-chyazic acid in solution. This liquid is colourless. I find that when kept it undergoes spontaneous decomposition. It ought, according to Vogel, to be kept in small phials quite filled with it. When newly prepared it has a peculiar pungent smell, reddens vegetable blues, and has an acid taste. It does not precipitate barytes water, and the precipitate which it occasions in acetate of lead is soluble in cold water.

Grotthuss made a very ingenious set of experiments to determine the composition of this acid. According to him the constituents are as follows:

3 atoms sulphur .... = 60·00
1 atom carbon .... = 7·54
1 atom azote .... = 17·54
3 atoms hydrogen ...... = 3·98

89·06

But this analysis, however ingenious, was not performed in a way sufficiently rigid to produce conviction. He shows that when sulpho-chyazate of potash is decomposed by sulphuric acid and heat, that sulphate of ammonia is formed. Hence he concludes that the azote and the hydrogen exist in sulpho-chyazic acid in the same proportion in which they exist in ammonia; or three atoms of hydrogen to one of azote. By decomposing a given weight of sulpho-chyazate of potash by means of chlorine, and ascertaining the weight of the sulphur, sulphuric acid, and carbonic acid evolved, he ascertained that the sulphur and carbon in sulpho-chyazic acid are to each other as the numbers 2·6 to 0·328. But these numbers are the same as 6 to 0·758. Now 6 is the weight of three atoms of sulphur, and 0·756 is very nearly the weight of an atom of carbon; so that this acid contains 3 atoms of sulphur and 1 atom of carbon, or at least the sulphur and carbon in the acid bear that ratio to each other. Having determined these two ratios, Grotthus ascertained the quantity of sulphur contained in this acid. The result of his experiment, which approaches very near that of Porrett, is that 100 parts of the acid contain 67·3 parts of sulphur. These were the data from which the constituents of the acid are estimated. The defective part of the deduction is the inference that the azote and hydrogen in the acid exist in the same ratio as they do in ammonia, merely because ammonia was formed when the acid was decomposed. Before such a conclusion can be admitted as demonstrated, it would be necessary to show that the whole of the azote and hydrogen is employed in the formation of ammonia, which Grotthus has not done. When cyanogen is decomposed by allowing it to stand dissolved in water, ammonia is formed, yet the azote and hydrogen do not exist in that substance in the same proportion as in ammonia. When concentrated nitric acid is made to act upon tin, ammonia is evolved; yet one of the constituents of it in this case is derived from the acid, and the other from the water.

Vogel has pointed out other inaccuracies in the mode of analysis adopted by Grotthuss, which destroys all the deductions which that chemist has endeavoured to establish. He himself is disposed to consider the acid as a compound of hydrocyanic acid and sulphur; but this cannot be established without a more rigid analysis than has yet been given. Mr. Porrett's analysis is by far the most ingenious and complete one which has yet appeared; but it is not quite satisfactory.

Grotthuss is of opinion that this acid is a hydracid, or a compound of hydrogen united to a base. To this supposed base, which however he did not succeed in obtaining in a separate state, he has given the unwieldy appellation of *anthrazothion.* I consider it as needless to make any observations on his names; as there is no great probability of their being adopted, at least in Great Britain; their enormous length alone would be an insurmountable objection. Indeed I think it ought to be laid down as a rule in chemistry, that the names of substances should not exceed two, or three or four syllables at most. This supposed base is considered by Grotthuss as a compound of all the constituents of sulpho-chyazic acid, except the hydrogen; namely,

3 atoms sulphur .... = 6·00
1 atom carbon .... = 0·75
1 atom azote .... = 1·75
8·50

But we have no evidence of the existence of this supposed base, at least in a separate state. I conceive it to be unnecessary to notice any of the many other important facts contained in the memoirs of Grotthuss and Vogel. I must refer the reader for farther information to the memoirs themselves as they are printed in the *Annals of Philosophy,* xiii. 39, 89, 101.*

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* Since the observations contained in the text were written, Mr. Porrett has published a new analysis of this acid, and has shown that his former notions respecting its composition were correct. We may therefore consider it as a com-

7. *Ferro-chyazic Acid.*—Mr. Porrett has discovered a method of procuring this acid in the state of crystals. He dissolves 58 grains of tartaric acid in alcohol, and mixes the solution with a solution of 50 grains of prussiate of potash, dissolved in two or three drachms of hot water. The potash and tartaric acid separate in the state of bitartrate of potash. The alcoholic solution retains only the ferro-chyazic acid, which, by spontaneous evaporation, is deposited in the state of small cubic crystals. (*Annals of Philosophy*, xii. 216.)

When I published my experiments on the composition of ferro-chyazic acid, I was far from considering the results which I obtained as precise, as will obviously appear to any one who takes the trouble to peruse the paper in question. But my reason for publishing them was, that they were as accurate as I could make them with the kind of apparatus which I employed. I have since modified and improved this apparatus considerably; but have not yet brought it to a degree of precision on which full confidence can be put. But I expect very soon to be provided with one possessed of all the requisite precision. As soon as this is the case, I shall repeat my experiments again, and flatter myself that the result will be more satisfactory. Mr. Porrett has favoured us with an analysis, which is certainly more likely to be accurate than mine, as it agrees with the atomic theory. But from the many experiments which I have made on the analysis of combustible substances by means of peroxide of copper, I am satisfied that we cannot venture to draw conclusions from one solitary experiment, without the utmost hazard of deceiving ourselves. I shall not make any observations on the weights of atoms used by Mr. Porrett in his calculations, though I have no doubt that they are less precise than those which I employ, because I propose to return to this subject in a future paper.

8. *Purpuric Acid.*—This is the name given by Dr. Wollaston to a new acid discovered by Dr. Prout, and formed by the action of nitric acid on uric acid. The process by which this acid may be obtained is as follows:

Dissolve pure uric acid in dilute nitric acid; after the solution is completed, saturate the excess of nitric acid with ammonia, and then slowly concentrate by evaporation. As the concentration advances, the liquid becomes dark coloured, and dark red granular crystals soon separate in abundance. These are crystals of purpurate of ammonia. These crystals are to be dissolved in

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pound of 2 atoms sulphur and 1 atom of hydrocyanic acid; or its constituents may be represented thus:

2 atoms sulphur...... ..... = 4
2 atoms carbon............. = 1·5
1 atom azote............... = 1·75
1 atom hydrogen............ = 0·125

7·375 = atom of sulpho-chyazic acid.—(See *Annals of Philosophy*, xiii. 356.)

caustic potash, and heat applied to the solution till the red colour entirely disappears. Drop the alkaline solution into dilute sulphuric acid, the purpuric acid separates in a state of purity.

Purpuric acid, thus obtained, is a cream-coloured powder, destitute of taste and smell. It is scarcely soluble in water, and not soluble in alcohol or ether. It dissolves in the concentrated mineral acids, but not in these acids when in a state of dilution, nor in solutions of oxalic, citric, and tartaric acid. Concentrated nitric acid dissolves it with effervescence, and if heat be applied, purpurate of ammonia is formed. Chlorine produces the same changes as nitric acid. It dissolves in concentrated acetic acid when assisted by heat.

It does not redden litmus paper, does not attract moisture from the atmosphere; but assumes a reddish colour, and is apparently converted into purpurate of ammonia. When heated, it neither melts nor sublimes, but acquires a purple colour, and then burns away without yielding any remarkable odour. When distilled, it yields carbonate of ammonia, a little prussic acid, and an oily looking fluid, while a pulverulent charcoal remains behind. Its constituents, as determined by heating it with peroxide of copper, were as follows:

2 atoms hydrogen. . . . . = 0·25
2 atoms carbon . . . . = 1·50
2 atoms oxygen. . . . . = 2·00
1 atom azote . . . . = 1·75

5·00

If this analysis be correct, the weight of an atom of purpuric acid is precisely the same with that of an atom of sulphuric acid; consequently the constitution of the sulphates and purpurates will be the same. This would require to be determined by analysis, before we can have any precise notion of the weight of an atom of purpuric acid.

Most of the purpurates have a red colour. Purpurate of ammonia crystallizes in four-sided prisms, which by transmitted light are deep garnet-red, but by reflected light appear of a brilliant green. Most of the other purpurates possess the same peculiarity. Purpurate of ammonia is soluble in about 1500 times its weight of water at 60°, but it is much more soluble in hot water. The solution has a slightly sweetish taste, no smell, and a fine crimson colour. Purpurates of potash and of magnesia are much more soluble than purpurate of ammonia or purpurate of soda. Purpurate of lime resembles in colour the crust of the lobster before boiling. Purpurates of lime, barytes, strontian, alumina, silver, and mercury, seem to be least soluble; while purpurates of gold, platinum, lead, zinc, tin, copper, nickel, cobalt, and iron, are most soluble.—(See Phil. Trans. 1818, p. 240.)

9. *Gingoic Acid.*—Gingko biloba is a name given by Linnæus to a tree from Japan, which was brought to England about the middle of the last century, and which has gradually made its way to all the other countries of Europe. It blossomed for the first time in England, and Sir James Edward Smith published a description of it under the name of Salisburya adianthifolia. This name has been adopted by Wildenow, and in the new edition of the plants in Kew Gardens; but almost all other botanists have retained the old name. M. Peschier has lately made some experiments on the juice obtained by expression from the fruit of this tree, which is about the size of a nut. Its taste is astringent; it reddens vegetable blues, and contains in it an acid which bears a close resemblance to the gallic acid; but which M. Peschier considers as possessing peculiar properties, and which, on that account, he has distinguished by the name of *gingoic acid.* The following table exhibits the comparative action of gallic acid obtained by sublimation, and of the juice of the fruit of the gingko upon different reagents.

Gallic Acid.	*Juice of the Fruit of Gingko.*
1. Precipitates calcareous salts.	1. Ditto.
2. Does not alter the salts of barytes, strontian, and magnesia.	2. Ditto.
3. Renders lime-water brown, without occasioning a precipitate.	3. Forms a white precipitate, which becomes gradually brown.
4. Forms a brown cloud in barytes-water, which is redissolved.	4. Forms a permanent brown precipitate, and the liquid remains brown.
5. With strontian water, the same.	5. Ditto.
6. Has no action on muriate of platinum.	6. Ditto.
7. Forms a brown precipitate in a solution of gold.	7. Forms a reddish-yellow precipitate, which becomes brown.
8. A brown precipitate in acetate of copper.	8. Ditto.
9. No action on sulphate, nitrate, and muriate of copper.	9. A brown precipitate in the nitrate and muriate, a slight greenish cloud in the sulphate.
10. Changes ammoniacal sulphate of copper to brown; but no precipitate.	10. Occasions a bluish-green precipitate.
11. Very little action on acetate of lead, and none on nitrate.	11. Forms white precipitates with all the solutions of lead.

Gallic Acid.	*Juice of the Fruit of Gingko.*
12. No action on salts of zinc.	12. Forms white precipitates in all the salts of zinc.
13. Ditto, with nitrate of silver and salts of manganese.	13. Precipitates nitrate of silver and salts of manganese white.
14. No action on mercurial salts.	14. A white precipitate in corrosive sublimate, and a canary yellow ditto in nitrate of mercury.
15. No effect on sulphate of iron at the instant of mixture; but the colour becomes amethystine, blackens in 24 hours, and lets fall a black precipitate.	15. Ditto; only the precipitate is not black, but continues amethystine.
16. Persulphate of iron thrown down of a deep blue.	16. Ditto precipitated green, and the liquid remains green.
17. Forms brown precipitates, with nitrate and muriate of iron, which redissolve, and give a brown colour to the liquid. In acetate of iron forms a very light black precipitate, which remains suspended several days.	17. Gives permanent brown precipitates in the nitrate and muriate of iron. With acetate of iron exhibits the same phenomena as gallic acid does.

(Bibl. Univers. vii. 29.)—

I think these experiments hardly warrant the inference drawn by M. Peschier, that this juice contains a peculiar acid. It is much more likely that the acid present is the gallic, and that the variations observable in the preceding table are owing to the presence of some other vegetable bodies in the juice of the gingko, which are of course wanting in the solution of gallic acid. Before the peculiar nature of this acid be established, it must be obtained in a separate state, and it must be shown that in that state it contains peculiar properties.

10. *Meconic Acid.*—This acid was first recognized by Sertürner in opium; but his account of it was defective, and on that account doubts have been thrown by some chemists upon its peculiar nature. M. Choulant has given us a very simple mode of preparing it, which will easily put it in the power of other persons to verify the statements of Sertürner respecting it. The process is as follows:

The infusion of opium is to be freed from morphia, and care must be taken that it does not contain any excess of ammonia. Into this infusion muriate of barytes is to be poured as long as any precipitate continues to fall. The precipitate, when well washed and dried, is pure meconate of barytes. Let it be triturated in a mortar with its own weight of glassy boracic acid, and heated sufficiently in a glass flask, the meconic acid sublimes in the state of fine white scales or plates (see *Annals of Philosophy*, xiii. 229). This acid, according to Choulant, possesses the following properties:

Taste, strongly acid, with an impression of bitterness. Soluble in water, alcohol, and ether. Reddens vegetable blues, and changes the solutions of iron to a *cherry-red* colour. When these solutions are heated, the iron is precipitated in the state of protoxide. From the experiments of Vogel, we learn that it is only on the persalts of iron that the meconic acid produces this colour. This property then is common to the meconic and sulphochyazic acid. Meconic acid, it would appear from the experiments of Sœmmering, is not of a poisonous nature.—(Ibid. xiii. 105.)

11. *Malic Acid.*—A very important set of experiments on the different substances, considered as containing malic acid, has been published by M. Braconnot. He examined the juice of apples, of the house-leek, &c. I consider these experiments to leave no doubt whatever that the malic acid of Scheele, when brought to a state of purity, is identical with the sorbic acid of Mr. Donovan. Of course there are not two distinct acids, as has been hitherto supposed, but merely one acid. It has been called malic acid when in a state of impurity, and sorbic acid when obtained in a sufficiently pure state. To Mr. Donovan then we are indebted not for the discovery of a new acid, but for pointing out a method of obtaining an old acid in a state of purity, and of course in such a state that its characters can be reognized and established. Since this is the case, it seems but fair to return again to the original name given to this acid by Scheele, who was undoubtedly the original discoverer of it, though he did not succeed in procuring it in a state of complete purity.

Malic acid then, when pure, is colourless, soluble in water, alcohol, and ether, and is capable of crystallizing. It is readily sublimed when heated, but the sublimed crystals possess characters somewhat different from those of malic acid before it has been exposed to heat. The acid, thus altered, has been called *pyro-malic acid.*

Malate of magnesia and malate of zinc crystallize readily; but malates of potash and soda are incapable of crystallizing.

Pure malic acid neither precipitates nitrate of lime, nor nitrate of silver, nor nitrate of mercury. With acetate of lead it forms a white precipitate soluble in distilled vinegar, and even in boiling water. It produces no sensible change when dropped into lime or barytes-water. Such readers as are interested in vegetable physiology should peruse the paper of Braconnot above alluded to. It is to be found in the Ann. de Chim. et Phys. viii. 149. A good deal of valuable information will be found likewise in M. Braconnot's paper on sorbic acid, and in that of M. Vauquelin on the same subject, of both which an account will be found in the *Annals of Philosophy*, xii. 290.

12. *Gallic Acid.*—M. Braconnot, who has turned a great deal of his attention to vegetable substances, has published a method of procuring gallic acid, which promises to be more economical and much more effectual than any of the processes hitherto proposed. It is founded on the original process of Scheele, which, however, Braconnot has shortened, and considerably modified. His process, as he has described it, is as follows:

Two hundred and fifty grammes of nutgalls were infused for four days in a litre of water (nearly half a pound avoirdupois of nutgalls in a wine-quart of water), taking care to agitate the mixture from time to time. The whole was then squeezed through a cloth, and the liquid passed through a filter. It was then left in an open glass caraf from July 22 to Sept. 22. No sensible quantity had diminished, but it had deposited a considerable quantity of crystals of gallic acid. These were separated by squeezing the liquid through a cloth. The liquid, when evaporated to the consistence of a syrup, deposited an additional quantity of crystals, which were separated in the same manner. The residual matter of nutgalls from which the infusion had been procured, when moistened with water, and left to spontaneous fermentation, yielded an additional crop of crystals when treated with hot water; so that nutgalls, when properly treated, yield the fifth part of their weight of gallic acid.

By these different processes, M. Braconnot obtained 62 grammes of gallic acid, still coloured, and mixed with an insoluble powder. It was boiled with three decilitres (18 cubic inches) of water, and filtered while boiling hot. The liquid on cooling deposited 40 grammes of crystals of gallic acid of a yellowish-white colour. The mother-water was brown, and when properly evaporated yielded 10 grammes more of crystallized gallic acid, darker coloured than the first crystals. To free these crystals entirely from colouring matter, they were mixed with eight times their weight of water and about the fifth of their weight of ivory black, and the mixture was kept for about a quarter of an hour at the boiling temperature. It was then filtered while hot. On cooling, it concreted into a mass of perfectly white crystals of gallic acid, which were separated from the liquid by pressure in a cloth.

The acid thus obtained is white, like snow, and quite pure. Its aqueous solution is not rendered muddy by a solution of glue. Its taste is weakly acid, and it leaves in the mouth an impression of sweetness.—(Ann. de Chim. et Phys. ix. 181.)

13. *Ellagic Acid.*—By this very absurd name (the French term *galle* reversed), Braconnot has thought proper to distinguish an acid substance which he extracted from nutgalls at the same time with gallic acid. Chevreul, in a note published in the Ann. de Chim. et Phys. ix. 329, informs us that he had given to the

world a pretty considerable number of experiments on this substance in the article Tannin, published in the chemical part of the Encyclopædie Methodique, in 1815; but that he had neglected to give it a name; nor does he seem to have been aware that it was entitled to be considered as a peculiar acid. This acid was obtained by taking the powder separated by filtering the solution of gallic acid obtained from the crystals that had formed spontaneously in the infusion of nutgalls. To free it from gallate of lime, &c. with which it was mixed, it was treated with a dilute solution of potash, which dissolved the acid with the evolution of a considerable quantity of heat, the solution had an intense yellow colour, and gradually let fall a pretty copious quantity of pearl-coloured powder, which was separated by the filter, and decomposed by dilute muriatic acid. The ellagic acid thus obtained is a white powder, with a slight shade of buff. It is insipid, and is not sensibly soluble in water even when boiling hot. It does not decompose the alkaline carbonates even when assisted by heat; but it unites with caustic soda, and potash, and destroys their alkaline properties. These salts are insoluble in water, but they become soluble if a little potash or soda be previously dissolved in that liquid. The solution is very dark buff coloured. The ellagate of ammonia is likewise insoluble, and does not become soluble even when an excess of ammonia is added. It separates the lime when agitated in lime-water. Nitric acid does not seem to act upon it at first, but it gradually gives that acid a red colour similar to that of blood. If the action be continued, a good deal of oxalic acid is formed.

It does not combine with iodine. When heated, it does not melt, but burns away with a sort of scintillation without emitting flame. When distilled, it leaves charcoal, and produces a yellow vapour, which condenses into transparent crystals, of a fine greenish-yellow colour. This sublimate is tasteless, and insoluble in water, alcohol, and ether; but it dissolves readily in a solution of potash, and communicates a yellow colour. In short, the yellow crystals possess nearly the characters of the ellagic acid itself.—(Ann. de Chim. et Phys. ix. 187.)

14. *Lampic Acid.*—This is the name by which Mr. Daniell has thought proper to distinguish a peculiar acid substance which Sir Humphry Davy recognized as formed when ether is decomposed by the continued action of a red-hot platinum wire. This acid was examined by Mr. Faraday, but upon too small a scale to admit of accurate conclusions. Mr. Daniell succeeded in obtaining it in considerable quantities by means of the well-known lamp without flame, or the spirit lamp, which keeps a coil of platinum wire red-hot by the slow combustion of alcohol or ether. He put this lamp in the head of an alembic, to which a receiver was adapted, and by keeping the slow combustion going for a considerable time (he mentions having continued it for six weeks at one time), he collected considerable quantities of

the acid liquor formed. The acid, he thinks, was the same whether obtained from ether, alcohol, or oil of turpentine. He collected about a pint and a half of liquid from the combustion of ether. It was a colourless liquid, of an intensely sour taste, and pungent odour, irritating the lungs, and producing effects similar to chlorine. Its specific gravity varied from 1·000 to 1·008. When evaporated carefully, it allows a quantity of alcohol to escape, and the specific gravity becomes 1·015. It reddens vegetable blues, and decomposes all the earthy and alkaline carbonates. He found the composition of lampate of soda and lampate of barytes as follows:

Lampate of soda.

Acid	62·1	6·554
Soda	37·9	4·000

Lampate of barytes.

Acid	40·2	6·556
Barytes	59·8	9·750

These two analyses correspond well, and indicate 6·555 as the equivalent number for lampic acid. All the lampates are deliquescent salts. Lampate of ammonia is very volatile, and evaporates at a heat below that of boiling water. When burnt, it emits a disagreeable smell, like that of burning animal matter. Gold, platinum, and silver, are reduced to the metallic state by this acid. I think it not unlikely that the effect is owing to the alcohol with which the acid is obviously mixed. Mr. Daniell analyzed it by the method invented by Gay-Lussac and Thenard. The constituents which he obtained were:

Carbon	40·7
Hydrogen	7·7
Water	51·6
	100·0

Hence he infers the constituents to be:

1 atom carbon . . . . = 0·75
2 atoms hydrogen . . . . = 0·25
1 atom oxygen . . . . = 1·00
2·00

It is sufficiently obvious that this analysis cannot be reconciled to the number 6·555, which was found to be that of lampic acid from the salts examined. Neither is 6·555 a multiple of 2·00. Of consequence the constitution must be different from what has been deduced by Mr. Daniell from his experiments. Peroxide of copper would furnish a much easier and more accurate mode of analysis. It is surprising that Mr. Daniell did not have recourse to it.—(Institution Journal, vi. 318.)

15. *Pyromucic Acid.*—This acid was obtained by M. Houton Labillardiere by distilling saclactic or mucic acid. The matter that comes over is to be mixed with four times its weight of water, and then evaporated to the requisite degree; the new acid is deposited in crystals, and new crystals may be obtained by concentrating the mother-liquor still further. When put into a retort, and heated to the temperature of 266°, they melt, and sublime in the form of yellow crystals, which, on being redissolved in water and crystallized, become perfectly white and pure.

Pyromucic acid is white, has an acid taste, but is destitute of smell. When heated to 266°, it melts, and sublimes, and condenses into a liquid which becomes solid on cooling. It does not deliquesce. It reddens vegetable blues; is more soluble in hot than cold water; it is more soluble in alcohol than water; it neutralizes the salifiable bases, and forms salts, most of which crystallize. Pyromucate of barytes is composed of

Acid	57·7	13·331
Barytes	42·2	9·750
	99·9	

When analyzed, by means of peroxide of copper, its constituents were found to be:

Carbon	52·118
Oxygen	45·806
Hydrogen	2·111
	100·035

The number of atoms coming nearest to these proportions, and to the weight of the equivalent number for the acid, as indicated by the composition of pyromucate of barytes, is as follows:

9 atoms carbon	= 6·75	50·94
6 atoms oxygen	= 6·00	45·28
4 atoms hydrogen	= 0·50	3·78
	13·25	100·00

(See Ann. de Chim. et de Phys. ix. 365.)—

16. *Rheumic and Zumic Acids.*—The existence of these two acids as peculiar bodies has been destroyed. M. Lassaigne has shown that the acid in the juice of the rheum ponticum is nothing else than oxalic acid.—(*Annals of Philosophy*, xiii. 71.)

Vogel has shown that the zumic acid possesses the characters of the lactic acid. If this statement be correct, the lactic acid is a product of the vegetable kingdom, or at least formed by the fermentation of vegetable bodies (Ibid. xii. 391). It is curious that this acid, to which Braconnot had given the name of nanceic acid, was called *zumic* acid both in this country and in Germany, about the same time, without any concert whatever between those who imposed that name.

The preceding sketch contains the discovery of no fewer than seven new acids; namely:

1. Hydrosulphurous acid.
2. Uranic acid.
3. Manganesic acid.
4. Purpuric acid.
5. Ellagic acid.
6. Lampic acid.
7. Pyromucic acid.

Three acids, hitherto considered as peculiar, have been shown to be the same with three others which have been long known.

(1.) Malic and sorbic acids have been shown to be the same.
(2.) Rheumic acid is merely oxalic acid.
(3.) Zumic acid is the same with lactic acid.

VI. ALKALIES.

This department of the science promises fair to be enriched likewise with a variety of new vegetable bases possessing the characters of an alkali. Sertürner's paper on morphia has drawn the attention of chemists to this subject, and the discovery of several new substances possessing similar properties has already rewarded their exertions.

1. The discovery of *lithina* by M. Arvedson was announced in the Historical Sketch for last year, and the properties of that new mineral alkali, as far as they had become known, were given. I had not at that time made any experiments on the analysis of petalite or spodumene, the two minerals in which it had been found; but it may, perhaps, be worth while to mention the methods of analysis which I have found successful. If petalite reduced to a fine powder be fused with nitrate or carbonate of barytes, it becomes soluble in muriatic acid. The earths may be separated in the usual manner by means of sulphuric acid and carbonate of ammonia, and the sulphate of lithina obtained is readily decomposed by carbonate of barytes. By this method, which appears sufficiently simple, the lithina may be obtained in a state of purity.

I do not know the method which M. Arvedson employed to obtain this alkali, as I have not yet seen his paper upon the subject. It is stated in the Institution Journal, vi. 226, that petalite may be analyzed in the usual way by means of potash. The muriate of lithina may be easily separated from the muriate of potash by means of alcohol, in which it is very soluble. I have not tried this method. To succeed by it, I suppose the alcohol must be strong; for muriate of potash is sensibly soluble in alcohol of the strength at which it is usually sold in the shops.

2. *Morphia.*—Choulant's method of obtaining this substance, of which an account has been given in the *Annals of Philosophy*, xiii. 153, seems better than any of the processes employed either by Sertürner or Robiquet.

Four ounces of pounded opium were digested in successive quantities of cold water till that liquid amounted to 16 pints. This infusion was evaporated on a sand-bath till it was reduced to eight ounces. The lime and the sulphuric acid which it contained in the state of sulphate of lime were then precipitated by oxalate of ammonia and muriate of barytes. The infusion was now diluted with eight pints of water, and precipitated by caustic ammonia. Upon the precipitate, an ounce of sulphuric ether was poured, and the whole was put upon a filter. A deep black liquid ran through by degrees, which weighed half an ounce. The morphia remaining on the filter was then digested three times in caustic ammonia, and as often in alcohol. Both of these liquids acquired a dark-brown colour. The morphia thus purified was dissolved in 12 ounces of boiling alcohol, and the filtered solution was set aside. It deposited transparent crystals of pure morphia, weighing 75 gr.

Morphia thus prepared is white and transparent. It crystallizes in octahedrons composed of two four-sided pyramids with square bases. It dissolves in 82 times its weight of boiling water, and the solution crystallizes on cooling. It dissolves in 36 times its weight of boiling, and in 42 times its weight of cold alcohol. It dissolves in eight times its weight of sulphuric ether. All these solutions change the infusion of Brazil-wood to violet, and the tincture of rhubarb to brown. They have a bitter and peculiar astringent taste, and the alcoholic and ethereal solutions when rubbed upon the skin leave a red mark. The equivalent number for the weight of this substance, from a mean of Choulant's analyses, seems to approach 8·25.

3. *Picrotoxine.*—This substance was detected by M. Boullay some time ago in the cocculus indicus, and I have given an account of its properties in the last edition of my System of Chemistry, iv. 55. Boullay has since shown that it is capable of neutralizing acids; of course it is entitled to be placed among the vegetable alkalies. It may be precipitated from the infusion of the cocculus indicus by caustic ammonia. If the precipitate be washed, and then dissolved in alcohol, it may be obtained by spontaneous evaporation in white silky needles.—(See *Annals of Philosophy*, xii. 312.)

4. *Vauqueline.*—This is a name given by MM. Pelletier and Caventon to a new vegetable alkali which they have extracted from the nux vomica, and from St. Ignatius's bean. Its properties are said to be as follows:

It is slightly soluble in water, very soluble in alcohol, gives a blue colour to litmus paper reddened by acids, does not redden turmeric, combines with acids, and neutralizes them, and forms with them crystallizable salts.—(*Annals of Philosophy*, xii. 314.)

VII. ANALYTICAL IMPROVEMENTS.

1. *Separation of Lime and Magnesia.*—Many attempts have

been made by chemists to discover a perfectly correct method of separating these two earths when they happen to occur together, as is the case in magnesian limestone, &c. I have mentioned some of these methods in the *Annals of Philosophy,* xii. 393. It was supposed that a solution of bicarbonate of potash poured into a muriatic or nitric solution of the two earths would precipitate the lime and leave the magnesia in solution. Bucholz demonstrated that this method is inaccurate, a portion of the lime being retained in solution, while a portion of the magnesia is precipitated.

The method proposed by Dobereiner is nearly similar to that practised long ago by Vauquelin, and occasionally practised by others. It consists in pouring carbonate of ammonia into the solution of these two earths; the carbonate of lime he assures us will be precipitated, while the magnesia forms a triple salt, and will be retained in solution. This method has been shown by Pfaff to be inaccurate. A part of the magnesia is always precipitated along with the lime.

Pfaff considers the best method of separating the two earths to be to neutralize the solution, and then to precipitate the lime by oxalate of ammonia. Mr. Phillips informs us, however, that the lime is never precipitated by this reagent till enough of the oxalate has been added to form a triple salt with the magnesia. In employing this method to separate the lime from sea-water, which I have frequently done, I have often been surprised at the slowness with which the lime was precipitated in such cases; but I have never examined whether the whole lime was precipitated. Indeed from the weight of the precipitate thus obtained, I think there is reason to suspect that a portion of the magnesia is precipitated as well as of the lime.

I mentioned in the number of the *Annals of Philosophy* above quoted the method which I had been in the habit of employing to separate these two earths from each other. I dissolve the two in muriatic or nitric acid, and add to the solution a sufficient quantity of sulphuric acid to decompose the muriates or nitrates formed. This mixture is now evaporated to dryness, and exposed to a heat sufficiently high to drive off the excess of sulphuric acid, if any be present. The dry mass is now digested in water to dissolve the sulphate of magnesia, and the water is mixed with a little alcohol to diminish the solubility of the sulphate of lime. By this method I have often obtained results which did not deviate materially from the truth.

Mr. Phillips has suggested a modification of this process, which I have not hitherto tried; but which promises to be an improvement. He dissolves the mixture of the two earths in muriatic or nitric acid; but instead of adding sulphuric acid, he adds sulphate of ammonia in sufficient quantity to convert the muriates or nitrates into sulphates. The sulphates thus formed are weighed. The mass is then digested in a saturated solution of sulphate of lime till all the sulphate of magnesia be dissolved. The sulphate of lime remaining is dried and weighed. Its weight subtracted from that of the original weight of the sulphates gives the sulphate of magnesia. The only part of this formula that requires verification, is that part of it in which it is supposed that when water saturated with sulphate of lime is digested in a mixture of sulphate of magnesia and sulphate of lime, it is incapable of dissolving any additional portion of sulphate of lime. Many salts have the property of increasing the solubility of others in water. It would be necessary, therefore, before giving full credit tó Mr. Philips' ingenious modification of my formula, to ascertain that sulphate of magnesia does not possess the property of increasing the solubility of sulphate of lime in water.—(Institution Journal, vi. 313.)

2. *Separation of Iron from Manganese.*—This is another analytical process, scarcely less difficult than the preceding. Many methods have been given, most of which I have tried without being fully satisfied with any of them. Gehlen's method of throwing down iron when in the state of peroxide, by means of succinate of ammonia, answers very well; but it is too expensive for common use, especially when the quantity of iron is considerable. Hisinger's substitution of benzoate of ammonia or of potash, is probably a great improvement; but I have not examined this method with sufficient care to enable me to form an opinion. Mr. Hatchett's method of throwing down the iron from manganese by ammonia, when both are held in solution by muriatic acid, answers very well for procuring manganese free from iron; but it is not so easy by means of it to determine exactly the proportions of manganese and iron in the solution; and in cases of complicated mineralogical analyses, this method cannot be put in practice at all.

Mr. Faraday has suggested two methods of separating these two metals from each other; neither of which I have yet tried; but they promise to furnish us with more precise analytical results than any which have been yet proposed. The first of his methods is this:

"To a mixed solution of iron and manganese, add solution of sulphate or muriate of ammonia; then pour in pure potash; the iron will be precipitated, but the manganese will remain in solution in the state of a triple salt."

Another method suggested by him is this:

Let the iron in solution be brought to the state of a peroxide; throw down the oxides of iron and manganese together, wash them by decantation, and digest them in a solution of sal-ammoniac with a little sugar; the manganese will be dissolved in what state soever of oxidation it is, but the iron will remain.—(Institution Journal, vi. 357.)

It seems unnecessary to allude to Grotthuss' method of separating these two metals by means of sulpho-chyazic acid (Annals *of Philosophy*, xiii. 50); as he acknowledges that it is an imperfect process.

VIII. SALTS.

The salts constitute, by far, the richest department of chemistry; many of them still remain to be examined. It is therefore in the power of every industrious chemist to add new facts to the science, almost at pleasure, by examining this easy, though somewhat neglected, department of chemistry. The additions made to this province of the science during the last year have not been very numerous; though some of them are of considerable importance.

1. *Saltpetre.*—The process followed in France for purifying saltpetre, at least in the manufactories carried on by government, is the following: The salt, such as it is procured from the saltpetre-makers, is dissolved in the fifth part of its weight of water. The common salt, which exists in too great quantity in it to be dissolved, is removed from the bottom of the vessel, and the scum which collects on the surface is skimmed off. The liquid is clarified by a solution of glue, and then poured, while boiling hot, into a large copper bason, where it is continually agitated till it becomes cold. By this means it is separated in very small crystals. These crystals are put into wooden boxes, and sprinkled with water till that liquid passes off pure. The object in view, in agitating the liquid during the crystallization, is to make the size of the crystals as small as possible, in order to enable the subsequent process of washing to carry off the whole of the mother liquor attached to the crystals, in which alone the foreign salts, constituting the impurities, exist. Saltpetre, purified by this process, contains about $\frac{1}{3000}$ part of its weight of common salt.—(See Longchamp, Ann. de Chim. et Phys. ix. 200.)

2. *Carbonate of Potash.*—Chemists are aware that one of the easiest methods of obtaining this salt in a state of purity, is to burn a mixture of nitre and cream of tartar. M. Guibourt has shown that the best proportions are two parts of bitartrate of potash and one part of saltpetre; and that the mixture should be thrown into a crucible, heated rather below redness. If the crucible be at a strong red heat, there is always formed a considerable portion of cyadide of potassium, and it is difficult to get rid of the hydrocyanic acid, which is formed when the substance is dissolved in water. To avoid the formation of this substance, it is better to expose the mixture of salts only to a heat below redness.—(Jour. de Pharmacie, 1819, p. 59.)

3. *Ferro-chyazate of Potash.*—This is the salt usually called triple prussiate of potash. It has a fine yellow colour and crystallizes in square tables, with bevelled edges; it is transparent, and when seen by transmitted light is green, by reflected light topaz yellow; its specific gravity is 1·833. Its taste is saline

and cooling, and by no means disagreeable. It may be easily split into plates, and is not brittle, like most other salts, but possesses a certain quantity of softness and pliability. When heated it gives out moisture, and assumes a white colour. In a red heat it becomes black and alkaline, but its acid is not completely dissipated nor decomposed. The solubility of this salt in water is as follows:

At 54°, 100 parts of dissolve	27·8 of the salt.
100	65·8
150	87·8
200	90·6

It is insoluble in alcohol and ether. It is decomposed by sulphuric and nitric acid; the acid being driven off and destroyed. Mr. Porrett considers this salt, in its crystallized state, as composed of

1 atom ferro-chyazic acid	8·5	50·75
1 atom potash	6·0	35·22
2 atoms water	2·25	13·43
	16·75	100·00

While the constituents of ferro-chyazic acid, according to him, are as follows:

4 atoms carbon	= 3·00
1 atom azote	= 1·75
2 atoms hydrogen	= 0·25
1 atom iron	= 3·50
	8·50

But these determinations must be viewed rather as ingenious conjectures, than as the actual result of experimental analysis.

4. *Borax.*—This salt usually comes to Europe in a crude state. It is afterwards purified in Europe, and for many years the process was practised exclusively by the Dutch. I take it for granted that the salt is now refined in Great Britain; though I have no personal knowledge of any such manufactory, and should esteem it a favour if any of my readers, who happen to be acquainted with the fact, would give us some information on the subject. MM. Robiquet and Marchand have published the following formula for refining this salt, which they assure us will answer perfectly.

The crude borax is to be put into a vessel and covered with eight or ten centimetres of water; it is to be allowed to macerate for some time, agitating occasionally. After five or six hours, about $\frac{1}{100}$th part of slacked lime is to be added, then the whole is to be agitated and left till next day. The borax is now separated by means of a cloth, and the crystals are to be rubbed and carefully wiped. This washing is to be repeated till the water comes off quite clean. The borax, thus prepared and dry, is to be dissolved in two and a half times its weight of water, adding a kilogramme of muriate of lime for every hundred weight of borax. The liquid is now to be filtered through a cloth, and evaporated to the requisite degree of concentration. It is then to be put into conical vessels, made of lead or white wood, and cooled as slowly as possible; for the transparency and regularity of the crystals depend upon the slowness of the cooling.—(Ann. de Chim. et Phys. viii. 359.)

We are indebted to M. Vogel for some observations on the action of borax and boracic acid on bitartate of potash. If three parts of the bitartrate and one part of borax be boiled for some minutes with a sufficient quantity of water, a portion of tartrate of lime subsides. By evaporating the liquid, what is called *soluble cream of tartar* is obtained. It dissolves in its own weight of water at 54¼°, and in half its weight of boiling water. Sulphuric, nitric, and muriatic acids decompose it but imperfectly. A similar compound is obtained by employing boracic acid instead of borax. Bitartrate of soda may be employed instead of bitartrate of potash; but the nature of these singular compounds is still very imperfectly understood.—(See *Annals of Philosophy*, xii. 113.)

5. *Carbonate and Hydrate of Lime.*—I have no very exact idea respecting the compound of carbonate and hydrate of lime, which Theodor von Grotthuss informs us is made by passing a strong current of carbonic acid gas through lime water. It is unnecessary to repeat the phenomena described by him here; the reader will find a translation of the account given by Grotthuss in the *Annals of Philosophy*, xiii. 51.

6. *Chloride of Lime.*—This is the technical name by which Mr. Tennant's bleaching salt must be distinguished in chemistry. It is a combination of chlorine and lime; about one atom of chlorine to two atoms of lime; but one half of the lime remains behind when the powder is digested in water; when heated, oxygen is disengaged, and the substance is changed into chloride of calcium. This, when dissolved in water, is converted into muriate of lime. The chlorine may be transferred from the lime to barytes, strontian, and probably also magnesia, by double affinity. Thus it appears that chlorine is capable of combining not only with the metals, but likewise with the oxides and salifiable bases. In this respect it resembles sulphur and phosphorus, which possess the same properties.

I may here notice that the account which Welther has given of the formation of oxymuriate of lime in the Ann. de Chim. et Phys. vii. 383, is inaccurate in several particulars. It is a mistake that chlorine will not combine with unslacked lime; but when such a combination takes place, heat is evolved, and unless the increase of temperature be prevented, the continuance of the combination is stopped. Indeed, when the heat gets considerable, the chlorine which has already combined with the lime, seems to be again expelled.

7. *Persulphate of Iron.*—The combinations of sulphuric acid and the peroxide of iron have been till lately almost entirely overlooked. I was fortunate enough to ascertain the existence of no fewer than four different salts composed of peroxide of iron and sulphuric acid, namely,

	Acid.	Peroxide.
1 Persulpate	1 atom	+ 1 atom
2 Tripersulphate	3	+ 1
3 Quadripersulphate ...	4	+ 1
4 Subbipersulphate ...,	1	+ 2

The first of these was a substance which was not susceptible of examination, as it was decomposed by being placed in contact with water. The tripersulphate is a reddish yellow deliquescent salt, having a very astringent taste, and very soluble in water. It does not seem capable of crystallizing. The perquadrisulphate forms transparent and colourless crystals, having much of the taste and the shape of alum crystals. It was discovered by Mr. Rennie. I afterwards examined and analyzed it, but did not succeed in obtaining it in crystals. Mr. Cooper has since ascertained the way by which it may be obtained in regular crystals, and has described its properties considerably in detail.—(See *Annals of Philosophy*, xiii. 298.) He calls it a perbisulphate; but it is obvious, from his analysis, that the constituents of his salt are the same as those of my perquadrisulphate. The reason of this difference in our names is, that Mr. Cooper considers the weight of an atom of peroxide of iron to be 5, while I, to get rid of the anomaly of the half atom, represent its weight by 10 or 5 × 2. I do not see any other way of reconciling the oxides of sodium, iron, nickel, and cobalt, with the atomic theory. To give iron as an example:

An atom of iron weighs 3·5:
Protoxide is composed of 3·5 iron + 1 oxygen = 4·5:
Peroxide of 3·5 iron + 1·5 oxygen = 5:
or 3·5 × 2 iron + 1·5 × 2 oxygen = 10.

If the weight of peroxide be 5, it is composed of an atom of iron, and an atom and a half of oxygen. If its weight be 10, it is composed of 2 atoms iron + 3 atoms oxygen; the constitution is the same; but the anomaly of the half atom disappears.

8. *Muriates.*—From the experiments of Sir H. Davy and his brother, it has been concluded that when the muriates are exposed to a strong heat so as to drive off the whole of the water which they contain, they are converted into metallic chlorides.

Thus common salt, by this treatment, becomes *chloride of sodium*. Davy, accordingly, states that these chlorides are incapable of being decomposed by vitreous phosphoric, or boracic acid; but that they are readily decomposed by these bodies, and muriatic acid gas disengaged, when their action is assisted by the presence of water. This assertion has been lately put to the test of experiment by M. Vogel, of Munich. He exposed muriate of barytes and pure phosphoric acid to a strong heat in two separate platinum crucibles, then mixed them together, and exposed the mixture to a red heat in a platinum tube; abundance of muriatic acid gas was disengaged. When muriate of tin and muriate of manganese were substituted for muriate of barytes, the result was the same. Hornsilver likewise yielded muriatic acid gas, but in smaller quantity than the other salts. Superphosphate of lime may be substituted for phosphoric acid with the same result. When boracic acid is heated in the same way in contact with the alkaline muriates, muriatic acid gas is likewise disengaged.—(Jour. de Pharmacie, 1819, p. 61.)

These experiments are directly contrary not only to the experiments of Davy, but likewise of Gay-Lussac and Thenard, the chemists, who first drew the attention of chemists to the true nature of chlorine.—(See Recherches Physico-chemiques, ii. 103.) Hence, before they can be admitted as accurate, they will be required to be verified by other experimenters. I think it most likely that the acids used by Vogel had not been quite freed from water. Whether it be possible to drive off all the water from them by heat is a question of rather difficult determination. As far as phosphoric acid is concerned, I should be disposed to answer in the negative.

To Vogel we are indebted likewise for a set of experiments to determine the action of sulphur on the muriates. The result was, that the following metalline muriates were decomposed when heated with sulphur:

Protomuriate of tin,
Muriate of copper,
Muriate of manganese,
Muriate of lead,
Muriate of antimony,
Protomuriate of mercury,
Permuriate of mercury.

Sulphurous acid gas, and in some cases sulphuretted hydrogen gas, were exhaled, and a metallic sulphuret formed. Hence it appears that the sulphur deprives the metal of its oxygen. It appears also in some cases to decompose the muriatic acid. The muriates of potash, soda, and barytes, are but very slightly acted on when treated in this manner.—(See *Annals of Philosophy*, xii. 393.)

IX. VEGETABLE BODIES.

This department of chemistry is still in a very imperfect state, though it is obviously advancing with considerable rapidity. The first part of its progress will undoubtedly greatly increase the number of vegetable bodies. Though it is likely that when accurate formulas are fixed for procuring each of these bodies in a state of purity, so as to enable us to compare the various vegetable principles with each other, their number will be considerably diminished, or at least they will come to be arranged under a small number of genera. I shall proceed to state the most important facts in the chemistry of vegetables which have been ascertained during the last year.

1. *Sugar.*—Mr. Daniell has published some valuable experiments and observations on sugar. (See Institution Journal, vi. 32.) He found that if Kirchhoff's process for converting starch into sugar was stopped before the saccharine change was completed, that the starch acquired the properties of gum. This was observed long ago by Kirchhoff himself, and was, indeed, the circumstance that led that chemist to the discovery of starch sugar. The object which he had in view was to convert starch into gum. On trying the action of sulphuric acid he succeeded in part; but the gum formed did not possess all the requisite qualities. Hopes were entertained that by prolonging the boiling, these qualities would be developed. The experiment was tried, but the starch was found to be converted into sugar instead of gum.

Mr. Daniell has found that the supposed gum into which sugar was converted by Mr. Cruikshanks, by treating it with phosphuret of lime, is nothing else than a compound of lime and sugar. Sugar and lime may be easily united by boiling them together in a sufficient quantity of water. It would appear from the experiments of Mr. Daniell, that when this compound is kept for a considerable time, the sugar is altered in its nature, and converted into gum. Lime is employed in the West Indies by the sugar boilers, and there is reason to believe that it is often employed in excess. Hence raw sugar always contains a portion of it. Now it has the property of converting *strong* sugar into *weak* sugar: that is to say, of changing sugar from a crystallized compound, consisting of grains easily separating from each other like sand, of a grey colour and transparent, into a clammy yellow sugar having the feel of flour. The use of lime in sugar refining, Mr. Daniell thinks, is to render the colouring matter more soluble, and of course more easily removed by water.

2. *Manna.*—Bouillon Lagrange informs us that manna consists of two distinct substances, one soluble in cold alcohol, another insoluble in cold, but soluble in hot alcohol. The first

substance he considers as very analogous to suga ; the second as peculiar. (*Annals of Philosophy,* xii. 153.) If these conclusions were accurate, Vauquelin's results, that manna is incapable of undergoing the vinous fermentation, could not be true.

3. *Starch.*—The blue colour produced upon starch by the action of iodine is now well known, and this re-agent is accordingly frequently employed to detect the presence of starch in vegetable bodies. M. Vincent has discovered that prussian blue is not without its action on starch. If four parts of starch and one of prussian blue be triturated in a mortar, and then boiled in a considerable quantity of water, it becomes green and then brown, and does not recover its blue colour, though treated with an acid. The liquor forms a fine prussian blue when mixed with equal volumes of sulphate of iron and solution of chlorine. It would appear that in this process the starch is altered in its nature, and converted into a kind of gum.—(See *Annals of Philosophy,* xiii. 68.)

4. *Colouring Matters of Vegetables.*—Respecting the nature of the substances to which the different colours in the vegetable kingdom are owing, very little satisfactory information has yet been acquired by chemists. Many of them are of so fugitive a nature, as to baffle every attempt to obtain them in an isolated state, while others, which are of a more permanent nature, cannot be easily freed from the various foreign bodies with which they are in combination. Mr. Smithson has favoured the world with a number of facts respecting these colouring matters, which though imperfect and isolated will not be without their utility.

The infusion of turnsol (*litmus*) contains no alkali, lime, nor acid; and its natural colour is blue. When the colouring matter of turnsol is burnt, it leaves a saline matter, which, with nitric acid, forms nitrate of potash. Mr. Smithson suspects that this colouring matter, like ulmin, is a compound of a vegetable substance and potash. The assertion of Fourcroy that the natural colour of turnsol is red, and that it contains carbonate of soda, he finds without foundation.

The colouring matter of the violet is blue, but is changed by acids to red. Mr. Smithson informs us that the same colouring matter exists in the petals of the red rose, in the petals of red cloves, in the red lips of the petals of the common daisy, of the blue hyacinth, hollyhock, lavender, the inner leaves of the artichoke, and numerous other flowers. It colours the skin of several plants, of the scarlet geranium, and pomegranate tree. Red cabbage, and the rind of the long radish, are coloured by the same principle. Mr. Smithson conceives that the acid which reddens the radish is the carbonic.

Mr. Smithson gives us likewise a series of experiments on sugar loaf paper, on the juice of the black mulberry, the petals of the corn poppy, &c. but they are of so miscellaneous a nature, that I must satisfy myself with referring the reader to the paper itself. (Phil. Trans. 1818, p. 110.)

What is called sap green is the inspissated juice of the berries of the buckthorn, ripe or half ripe. It differs entirely from the green matter of vegetables in general, being soluble in water, and being rendered yellow by alkalies, of which it is a very sensible test. Acids render it red.

I may here notice Dr. Clarke's experiments, to demonstrate that iron constitutes the colouring matter of the red rose. (See *Annals of Philosophy*, xii. 196 296.) I have no doubt that Dr. Clarke extracted iron from the petals of the rose. Indeed I saw a small globule of iron which he had actually extracted; but a little consideration will be sufficient to convince us that the red colour of the rose cannot be ascribed to iron. The changes produced upon the colour of the rose by acids and alkalies, and the very fugitive nature of that colour, are quite inconsistent with the idea that the colour is owing to iron.

5. *Morphia.*—Sertürner has been generally considered as the discoverer of morphia. There can be no doubt that he first drew the attention of chemists to it, and gave it a degree of importance which it did not possess before, by showing that it possessed the properties of an alkali. But Vauquelin has shown, in a satisfactory manner, that the substance itself had been obtained by Seguin, and that most of its properties had been described by him many years ago in a paper communicated to the Institute in 1804; but not published till it made its appearance in the Annales de Chim. in December, 1814. In that paper Seguin showed that the alkalies precipitated a white matter from infusion of opium, which was soluble in hot water and in alcohol, which, crystallized in prisms, and dissolved in acids, but was not capable of combining with any alkaline body. These properties, so far as they go, belong to morphia, and serve to characterise it. We must admit, therefore, that Seguin first discovered morphia; but it is to Sertürner that we owe the first ideas respecting its alkaline nature.—(See Ann. de Chim. et Phys. ix. 282.)

6. *Camphor.*—This substance melts at 349°, and boils when heated to the temperature of 399°, as we are informed by Gay-Lussac. It is purified by mixing it with some quicklime, putting the mixture into a glass shaped somewhat like a phial, melting it, causing it to boil slowly, and keeping the upper part of the vessel at such a temperature that the camphor becomes solid, but retains a temperature not much under that at which it fuses. This high temperature is requisite, in order to give camphor that semitransparency which it is required to have by those that purchase it. Gay-Lussac has proposed as an improvement, in this troublesome and expensive process, to distil it in a retort like a liquid, and to condense it in globular copper receivers. This method would be much more rapid and less ex-

pensive, and would leave the camphor in the peculiar semitransparent state which it is at present required to possess.—(See Ann. de Chim. et Phys. viii. 75.)

7. *Action of Alcohol on Oil of Bergamot.*—It is a common practice with dealers in perfumes to adulterate oil of bergamot with alcohol. This induced M. Vauquelin to make a set of experiments on the action of these two bodies on each other, that it might be in his power, when necessary, to detect the fraud. The following are the results of these trials. (1.) Oil of bergamot may contain eight per cent. of alcohol of the specific gravity 0·817, without its being perceptible when it is mixed with water. (2.) When it contains a greater quantity, the surplus separates, dissolving about $\frac{1}{3}$d of its volume of oil. (3.) A small quantity of water, mixed with the alcohol, diminishes remarkably its action on the oil; for alcohol of the specific gravity 0·880 dissolves only $\frac{1}{28}$th of its volume, while pure alcohol dissolves almost half its volume. (4.) When alcohol of the specific gravity 0·847 is mixed with oil of bergamot of the specific gravity 0·856, the alcohol sinks to the bottom, and the oil swims on it. The reason is, that the oil, absorbing a portion of the alcohol, becomes lighter, while the residual alcohol, becoming weaker, increases in specific gravity.—(See *Annals of Philosophy*, xii. 150.)

8. *Oil of Carapa.*—This is an oil extracted in Cayenne from the fruit of a tree, called *carapa* by the natives and persoonia by botanists. This oil is solid at 39°; it melts when heated to 50°; it has an amber colour, which is more intense when the oil is liquid, than when it is solid. This oil has a taste so intensely bitter, that it cannot be used for any other purpose than burning in lamps. This bitter principle cannot be separated by water, alcohol, ether, or acetic acid. Neither is it completely removed by combining the oil with alkalies.—(See Cadet, Jour. de Pharmacie, 1819, p. 49.)

9. *Potash.*—Dr. Peschier, of Geneva, has pointed out a very ingenious way of detecting the presence of potash in vegetable juices or infusions, without the necessity of subjecting them to incineration. He introduces into the liquid in question a quantity of magnesia, and agitates for some time; the acid (usually carbonic, oxalic, or tartaric) forms an insoluble compound with the magnesia, while the alkali, thus set at liberty, remains in solution in the liquid, and may be detected by its properties.—(*Annals of Philosophy*, xii. 336.)

10. *Sugar and Gum in Potatoes.*—Dr. Peschier has also detected the presence of mucous sugar, and of gum in the potato. Hence we see the reason why it undergoes the vinous fermentation.—(*Ibid.* p. 337.)

11. *Rice.*—Vauquelin made a set of experiments, chiefly with a view to ascertain whether rice contained any saccharine matter; but he was not able to discover any. He detected a little

phosphate of lime and some gum; but found this grain composed almost entirely of a peculiar kind of amylacious matter or starch. - (*Annals of Philosohpy*, xii. 151.)

12. *Anthemis Pyrethrum, or Pellitory of Spain.*—This substance, when chewed, excites a burning sensation in the mouth, and occasions a copious flow of saliva. It was examined by M. Gautier with a view to ascertain the nature of the substance which occasions this peculiar sensation. He found it to be a peculiar fixed oil residing in the bark, and which, on a close inspection, may be seen lodged in it in minute vesicles. This oil has a reddish colour, a strong smell, is insoluble in water, solid while cold, but melting when heated. The following are the substances which Gautier extracted from the pyrethrum:

Volatile oil, a trace.	
Fixed oil	5
Yellow colouring principle	14
Gum	11
Inulin	33
Muriate of lime, a trace.	
Woody matter	35
	98

The inulin had been previously observed in this vegetable substance by Dr. John. Gautier found that iodine gave it a yellow colour, instead of the blue colour which it communicates to starch.—(Ann. de Chim. et Phys. viii. 101.)

13. *Chenopodium Olidum.*—This plant contains uncombined ammonia, to which, in the opinion of MM. Chevalier and Lasseigne, it owes its peculiar smell. The substances extracted from the plant by these chemists, were the following:

Carbonate of ammonia,
Albumen,
Osmazome,
An aromatic resin,
A bitter matter,
Nitrate of potash, in considerable quantity,
Acetate and phosphate of potash,
Tartrate of potash.

100 parts of the dried plant yield 5½ of potash.—(See *Annals of Philosophy*, xii. 231.)

14. *Juice of the Bilberry.*—The juice of vaccinium mystillus, or bilberry, contains a colouring matter, citric and malic acids, and a considerable quantity of uncrystallizable sugar. When diluted with an equal bulk of water, and mixed with yeast, it ferments readily, and forms alcohol in considerable quantity. Charcoal or clay removes the colouring matter completely from this juice, and renders it as limpid as water.—(Vogel, *Annals of Philosophy*, xii. 232.)

15. *Colouring Matter of Red Wine.*—This colouring matter is distinguished from every other upon which M. Vogel has made experiments by the property which it has of forming a greenish grey precipitate with acetate of lead. By this property we can distinguish whether a red wine is genuine or factitious.—(*Ibid.* p. 232.)

16. *Lathyrus Tuberosus.*—The tubercles of this plant are cultivated in Holland, and used as an article of food. They are called by the French macjon. They were lately subjected to a chemical analysis by Braconnot, who found 500 parts of them to yield the following constituents:

Water	327·98
Starch	84·00
Sugar (identical with common sugar)	30·00
Woody fibre	25·20
Animalized matter	15·00
Albumen	14·00
Oxalate of lime	1·80
Rancid oil Adipocere	0·90
Phosphate of lime	0·50
Sulphate of potash	0·22
Malate of potash	0·20
Phosphate of potash	0·10
Muriate of potash	0·10
Odorous principle.	
	500·00

—(Ann. de Chim. et Phys. viii. 241.)

17. *Menispermum Cocculus.*—Boullay has published a new set of experiments on this substance, from which he had previously extracted the substance, to which he has given the name of *picrotoxine.* From these new experiments it appears,

(1.) That the picrotoxine is not only a new substance, and a very dangerous vegetable poison when in the pure and crystallized state; but likewise a salifiable base, capable of neutralizing acids, and of forming well characterized salts. The sulphate of picrotoxine, for example, is composed of

Acid	10	5
Picrotoxine	90	45
	100	

From this composition it would appear that the equivalent number for picrotoxine is 45. It is therefore the heaviest of all the alkaline bodies, and of course capable of saturating the smallest quantity of acids of any salifiable base at present known.

(2.) The vegetable acids are the best solvents of this poisonous substance, and most proper to neutralize its deleterious action.

(3.) The fruit of the menispermum cocculus contains likewise a peculiar acid, to which Boullay has given the name of *menispermic* acid. It possesses some properties analogous to those of the malic acid; but it is distinguished from the other vegetable acids by the property which it has of precipitating persulphate of iron green, and sulphate of magnesia white. The menispermate of magnesia is not decomposed by sulphuric acid. This acid does not readily crystallize; nor is it converted into oxalic acid by the action of nitric acid.

(4.) The fruit of the menispermum cocculus contains two peculiar fixed oils, possessing different properties, and having a different consistence.

(5.) It appears to contain likewise a quantity of sugar.—(See Jour. de Pharmacie, 1819, p. 1.)

18. *Lichen Praxineus.*—M. Cadet has subjected a lichen from Teneriffe, said to be employed as a red dye, to some experiments; but did not succeed in extracting any thing from it likely to be of use to European dyers. By treating the lichen successively with ether, alcohol, and water, he extracted the following constituents:

(1.) A reddish yellow colouring matter, soluble in water.

(2.) A fatty substance, soluble in ether, but insoluble in alcohol; rendered by the alkalies soluble in water; but at the same time changing its colour.

(3.) A resin soluble in alcohol; but precipitated by water.

(4.) An extractive matter.

(5.) A salt with base of lime.

(6.) A very small quantity of mucilage.—(*Ibid.* p. 54.)

X. ANIMAL SUBSTANCES.

1. *Cochineal.*—MM. Pelletier and Caventon have published an elaborate set of experiments on the cochineal insect. They found it composed of the following substances:

(1.) Carmine.

(2.) A peculiar animal matter.

(3.) A fatty matter composed of { steärine, elaïne, and an odorant acid.

(4.) The following salts: phosphate of lime, carbonate of lime, muriate of potash, phosphate of potash, potash united to an animal acid.

The substance to which these chemists have given the name of *carmine*, is the colouring matter of the insect. John had already made some experiments on it, and had given it the name of *cochinealin*; but it would appear from the result of the experiments of Pelletier and Caventon, that John did not succeed in obtaining this substance in a state of purity. Hence the characters by which he has distinguished it are not accurate. The method which Pelletier and Caventon took to obtain the *carmine* in a state of purity was to digest the cochineal insect in alcohol, as long as it gave a red colour to that liquid. These solutions, when left to spontaneous evaporation, let fall a crystalline matter of a fine red colour, consisting of the *carmine;* but not in a state of purity. To obtain it pure, these crystals were dissolved in strong alcohol, and the liquid was mixed with its own bulk of sulphuric ether. It became muddy, and after an interval of some days the carmine was deposited at the bottom of the vessel, forming a beautiful purplish red crust; the liquor was become perfectly clear, and had a yellowish red colour. The properties of the carmine thus obtained are as follows:

It has a fine purple red colour. It adheres strongly to the sides of the vessel in which it is deposited. It has a granular appearance, as if it were composed of crystals. It is not altered by exposure to the air. It does not absorb any sensible quantity of moisture. When heated to the temperature of 122° it melts. If the heat be increased, it swells up and is decomposed, yielding carburetted hydrogen gas, a great deal of oil, and a little water, having a slightly acid taste. It gives out no traces of ammonia.

It is very soluble in water. The liquid may be reduced by evaporation to the consistency of a syrup; but the carmine does not crystallize. The watery solution of it has a fine carmine red colour. A very small portion of this substance gives a strong colour to a great quantity of water. It is soluble likewise in alcohol; but the stronger the alcohol is, the worse a solvent does it become. It is insoluble in sulphuric ether; the weak acids dissolve it; but probably merely in consequence of the water which they contain. No acid precipitates it when pure; but they almost all throw it down when it is in combination with the peculiar animal matter of the cochineal; but all the acids produce a sensible change upon the aqueous solution of carmine. They make it assume in the first place a lively red colour, which gradually assumes a yellowish tinge, and at last becomes entirely yellow. When the acids are not very concentrated, the carmine is not altered in its nature; for when the acid is saturated, the colour resumes its former appearance.

Concentrated sulphuric acid destroys and chars carmine. Muriatic acid decomposes it without charing it, and converts it into a bitter substance, which has no resemblance to carmine. Nitric acid decomposes it with still greater rapidity. Some needle-form crystals are formed similar in appearance to oxalic acid; but they do not precipitate lime-water even when mixed with ammonia. The nature of these crystals was not ascertained.

Chlorine acts with energy on carmine, giving it first a yellow colour, which it gradually destroys altogether, It occasions no precipitate in aqueous solution of carmine if no animal substance be present. It is, therefore, a useful reagent to enable us to discover the presence of animal matter in this colouring principle. Iodine acts in the same way as chlorine, but with less rapidity.

When the alkalies are poured into a solution of carmine, they give it a violet colour. If the alkali be saturated immediately, the original colour appears, and of course the carmine remains unaltered, or at least only slightly modified; but if the action of the alkali be prolonged, or if it be augmented by the application of heat, the violet colour is dissipated, the liquid becomes first red, and then yellow. The nature of the colouring matter is now completely altered.

Lime-water occasions a violet-coloured precipitate when dropped into the aqueous solution of carmine. Barytes and strontian occasion no precipitate, but produce the same change of colour as the alkalies. Alumina has a very strong affinity for carmine. When newly precipitated alumina is put into an aqueous solution of carmine, the liquid is totally deprived of its colour, and the alumina converted into a beautiful lake. If a few drops of acid be added to the aqueous solution before adding the alumina, the lake obtained has a fine red colour as before; but it becomes violet upon the application of the least heat. The same effect is produced by adding to the liquid a few grains of an aluminous salt.

Most of the saline solutions alter the colour of the aqueous solution of carmine; but few of them are capable of producing a precipitate in it. The salts of gold alter the colour merely; nitrate of silver produces no change whatever; the soluble salts of lead render the colour violet; and acetate of lead occasions a copious violet precipitate. By decomposing this precipitate by means of a current of sulphuretted hydrogen, we may obtain the carmine dissolved in water in a state of purity.

Protonitrate of mercury throws down a violet precipitate. Pernitrate of mercury does not act so powerfully, and the colour of the precipitate is scarlet. Corrosive sublimate produces no effect whatever.

Neither the salts of copper nor of iron occasion any precipitate; but the former changes the colour of the liquid to violet, the latter to brown.

Protomuriate of tin throws down a copious violet precipitate. The permuriate changes the colour to scarlet, but produces no precipitate. When gelatinous alumina is added to the mixture, we obtain a fine red precipitate, which is not altered by boiling.

None of the aluminous salts occasion a precipitate, but they change the colour to carmine. The salts of potash, soda, and ammonia, change the colour of the liquid to carmine red.

From the action of the different salts upon this colouring matter, Pelletier and Caventon have drawn as a conclusion that the metals susceptible of different degrees of oxygenation act like acids upon the colouring matter of cochineal when at a maximum of oxidation, but like alkalies when at a minimum or medium degree; and that this alkaline influence may be exercised in the midst of an acid when the oxides in question are capable of forming an insoluble precipitate with the colouring matter.

Tannin, and astringent substances in general, do not precipitate the colouring matter of cochineal.

Pelletier and Caventon mixed a quantity of the colouring matter with black oxide of copper, and subjected the mixture to the requisite degree of heat. The only gaseous substance obtained was carbonic acid. Hence it follows that carmine is composed of carbon, oxygen, and hydrogen, and that it contains no azote whatever.

The peculiar animal matter of cochineal has a good deal of resemblance to gelatine; but it is distinguished by peculiarities which render it necessary to consider it as a peculiar substance. But for a minute account of the characters of this body, and of the fatty matter of cochineal, I must refer the reader to the dissertation of Pelletier and Caventon itself.—(See Ann. de Chim. et Phys. viii. 250.)

2. *White Matter deposited by the Aphis Euonymus.*—M. Lasseigne collected a quantity of white matter deposited upon the leaves of the Euonymus Europæa by an aphis that inhabits these leaves, and subjected it to the following trials:

(1.) It was white, without smell; but having a pleasantly sweet taste. Cold water dissolved it readily. Cold alcohol did not act upon it; but hot alcohol dissolved it, with the exception of some white flocks possessing the characters of albumen. On cooling, the alcohol allowed the substance to precipitate in small, white, brilliant grains, which had a sweet taste.

(2.) These grains, when heated in a retort, produced a quantity of very acid oil.

(3.) The aqueous solution was not precipitated by acetate or subacetate of lead, nitrate of silver, or nitrate of mercury. Neither was any precipitate produced by the alkalies, the infusion of nutgalls, or the aqueous solution of chlorine.

(4.) Nitric acid converted it into oxalic acid.

(5.) When mixed with yeast, it gave no indication of fermentation.

From these properties, M. Lasseigne considers it as a species of manna.—(Jour. de Pharmacie, 1818, p. 526.)

3. *Gas in the Abdomen, and Intestines of an Elephant.*—The gas in the abdomen of this animal after death was found to be a mixture of carbonic acid and azotic gas, with a little sulphuretted hydrogen. The gas in the intestines, on the other hand, seemed a mixture of carbonic acid and carburetted hydrogen.—(Vauquelin, *Annals of Philosophy*, xii. 119.)

4. *Sinovia of the Elephant.*—This liquid was examined by Vauquelin, who found its nature to correspond nearly with that

of the sinovia of the ox examined many years ago by Margueron. —(*Annals of Philosophy*, xii. 120.)

5. *Eggs of the Pike.*—A portion of these eggs was washed in a large quantity of water. The water was evaporated, and a white coagulable substance was procured, which was completely soluble in caustic potash, and precipitated by the infusion of nutgalls and nitric acid. This substance being calcined, the salts which it contained were obtained. The matter itself was considered as albumen, and the salts were potash, phosphate of potash, muriate of soda, and phosphate of lime.—(Vauquelin, *Annals of Philosophy*, xii. 148.)

6. *Urine of Amphibious Animals.*—Dr. Prout ascertained some years ago that the urine (if that name can be given to a solid excrementitious substance) of the boa constrictor consisted entirely of uric acid. Since that time, the urine of different species of serpents has been examined by Dr. John Davy. When thrown out, it has a butyraceous consistence, but becomes quite hard by exposure to the air. It was always found to be uric acid, in a state nearly pure. He found also the urine of lizzards to consist of nearly pure uric acid. That of the alligator, besides uric acid, contains a large portion of carbonate and phosphate of lime. The urine of turtles was a liquid containing flakes of uric acid; and holding in solution a little mucus and common salt; but no sensible portion of urea.—(*Annals of Philosophy*, xiii. 209.)

7. *Calculi*—We owe to M. Lassaigne the analysis of the following calculi and animal concretions:

(1.) *Calculus from the Urinary Bladder of a Dog.*—It had a brown colour, an irregular figure, and was of the size of a nut. It was composed of urate of ammonia, mixed with a little phosphate of lime.

(2.) *Urinary Calculi of Oxen.*—They were composed of carbonates of lime and magnesia.

(3.) *Salivary Calculus of a Cow.*—This calculus was white, very hard, capable of being polished, about the size of a pigeon's egg, and its nucleus was an oat. It consisted of carbonate of lime, mixed with a little phosphate of lime, and some animal matter.

(4.) *Salivary Concretion from a Horse.*—It was white, soft, elastic, and had exactly the form of the canal in which it was lodged. Cold water extracted from it a little albumen with some carbonate and muriate of soda. Boiling alcohol extracted a trace of fat. Solutions of caustic potash and soda dissolved it with facility. When calcined in a platinum crucible, it was decomposed, exhaling the odour of burning horn, and left a little white ash, composed of carbonate and muriate of soda, and phosphate of lime. These facts show us that it was composed of mucus, with some albumen, and the salts stated to have been extracted from it.

(5.) *Concretion from the Brain of a Horse.*—It was white, slightly soft, and of the size of a nut. Boiling alcohol dissolved only a portion of it. On cooling, the alcohol let fall a white matter in plates, and of a fine pearly lustre. This substance did not stain paper like tallow. It melted at the temperature of 276°, and on cooling crystallized in brilliant plates. When digested in caustic alkalies, it underwent no alteration. Hence it is the substance found in human biliary calculi to which Chevreul has given the name of *cholesterine.*

The portion of the concretion insoluble in alcohol was composed of albumen and phosphate of lime.

(6.) *Concretions from the Lungs of a Cow labouring under Phthisis Pulmonalis.*—They had the form of small white grains, very hard, and united together by a mucous membrane. Weak nitric acid dissolved them with a slight effervescence. Ammonia threw down a copious precipitate from the solution, and oxalate of ammonia occasioned a slight precipitate. Hence the concretions consisted of phosphate of lime mixed with a little carbonate.

(7.) *Concretions found in a Cavity in the Mesentery of a Bull attacked with Phthisis.*—Their composition was precisely the same as that of the preceding.

(8.) *Matter found in a Schirrus situated in the Meso-colon of a Mare.*—This substance was yellowish, greasy to the feel, had the odour of rancid oil, and stained strongly blotting paper. It was a mixture of albumen and a peculiar matter, consisting partly of cholesterine, and partly of a white substance, crystallizing in needles, and reddening vegetable blues. When calcined, the concretion yielded phosphate and carbonate of lime.—(Ann. de Chim. et Phys. ix. 324.)

8. *Sulphate of Zinc devoured by Spiders.*—For the knowledge of this fact, one of the most curious yet observed, as connected with the food of the insect tribes, we are indebted to the sagacity of Mr. Holt. A quantity of sulphate of zinc, which he kept in a paper, disappeared, except a small external crust, in the centre of which was a large spider. To determine whether this insect, of the species called *aranea scenica,* had devoured the salt, he was put in a box with fresh sulphate of zinc, which he devoured in the same manner, converting it into a yellowish-brown powder. This matter was found lighter than the sulphate of zinc, from which it had been formed by the spider. It was insoluble in water, and appeared to have been deprived of a portion of its acid.—(See *Annals of Philosophy,* xii. 454.)

II. MINERALOGY.

This branch of natural history is divided into two departments; namely, *oryctognosy,* and *geognosy;* or *geology.* The second of these has become of late years a fashionable study in Great Britain and America; and numerous essays, containing geological descriptions of different tracts of country, more or less in detail, have made their appearance. Oryctognosy is at present little better than a chaos of confusion. I wish some person competent to the task would set about a mineralogical arrangement of minerals. Nothing can be conceived more imperfect than the Wernerian classification. Haüy has been more successful in determining the species. But orders and genera may be said to be entirely wanting in his system.

I. ORYCTOGNOSY.

I. *New Mineral Species.*

The mineral kingdom has been examined with so much industry for these last 40 years, that the discovery of new species must of necessity be a more difficult, and consequently a rarer occurrence than it formerly was. Werner, a short time before his death, amused himself with giving new names to several varieties of minerals, and constituting them into new species. An account of several of these, by M. Cordier and by Mr. Heuland, may be seen in the *Annals of Philosophy*, xii. 310 and 453. It may be requisite to notice the most remarkable of these here.

1. *Egeran.*—This mineral was named by Werner from Eger, in Bohemia, the place where it was discovered. By the kindness of Mr. Heuland, I have had an opportunity of examining various specimens of it. It possesses all the essential characters of *idocrase*, and must, therefore, as Cordier has observed, be considered as merely a variety of that species, differing chiefly in colour and opacity.

2. *Albin.*—This mineral, so called by Werner from its white colour, occurs at Mariaberg, near Aussig, in Bohemia, imbedded in clinkstone. Mr. Heuland has rightly observed, that it is a variety of apophyllite, and not of mesotype, as Cordier states it to be. These two species differ from each other very materially in their composition; the mesotype containing a good deal of alumina, while the apophyllite contains none. The alkali in the former is soda, in the latter potash.

3. *Pyrgom.*—This name was given by Werner to a mineral found in the valley of Fassa, and already distinguished by the Italian mineralogists by the name of fassaite. Nothing can be more different than the external appearance of this mineral and of common *augite*; yet in its crystalline form, the agreement is complete. Hence there can be no doubt that the two minerals belong to the same species.

4. *Gehlenite.*—This is a name given by Fuchs to a mineral discovered in the valley of Fassa, of which I have given a description in the last edition of my System of Chemistry (vol. iii. p. 329). From its appearance I was led to suspect that it was intimately connected with andaluzite; but Cordier's conjecture, that it is a variety of idocrase, is much more probable, as is obvious from a comparison of the constituents of the two minerals, according to the best analyses hitherto made:

	Idocrase.	Gehlenite.
Silica	35·50	29·64
Alumina	22·25	24·80
Lime	33·00	35·30
Oxide of iron	7·54	6·50
Oxide of manganese	0·25	—
Water	—	3·30

The first of these analyses was by Klaproth, the second by Fuchs. They do not differ more from each other than two different analyses of the idocrase by Klaproth do.

5. *Helvin.*—This mineral, as we are informed by Mr. Heuland, is found in Brother Lorenz's mine, near Schwartzenberg, in the Saxon Erzgebirge. It was named, it seems, from ἡλιος, the sun, on account of its pale yellowish-brown colour. Its primitive form is the regular tetrahedron. It is softer than glass, and melts before the blow-pipe into a blackish-brown glass. It has not yet been subjected to analysis. Hence it has not been determined whether or not it constitutes a peculiar species.

6. *Pelium.*—This mineral, so named by Werner from its blue colour (πελὸς, livor), occurs at Bodenmais, in Bavaria. It is crystallized in six-sided prisms, truncated on the edges and angles. Cordier says, that in other respects it perfectly resembles the dichroite.

7. *Skorodite.*—This mineral has been so named by Mr. Breithaupt, of Freyberg (from σκοροδον, garlic); because, when exposed to the heat of the blow-pipe, it gives out a garlic smell. It has been found at Slamm Asser, near Schneeberg, in Saxony. Mr. Heuland, from its external appearance, considers it as a cupreous arseniate of iron.

8. *Tungstate of Lead.*—This new species of lead ore has been found at Zinnwalde, in Bohemia. It greatly resembles the brown acicular phosphate of lead from Poullouen, in Britany; but it crystallizes in very acute four-sided pyramids.

9. *Knebelite.*—Dobereiner has thought proper to distinguish by this name a mineral of which a description will be found in the *Annals of Philosophy*, xii. 392. From his analysis, it appears to be a combination, or mixture, of an atom of silicate of iron with an atom of silicate of manganese. It ought, if this be the case, to be called silicate of *iron-and-manganese*. The characters given of this mineral are hardly sufficiently precise to characterize it. I have little doubt that it has been hitherto confounded under the name of *grey ore of manganese*. Whoever will undertake to investigate the chaos of minerals at present confounded under that name, will undoubtedly discover several new species of minerals, and throw light upon the varieties of an ore at present of very considerable importance to some of the most interesting branches of our manufactures.

10. *Tennantite.*—This is a name given by Messrs. William and

Richard Phillips to an ore of copper found in the mines of Dolcoath, Cook's Kitchen, and Tincroft, near Redruth, and in Huel Virgin, Huel Unity, and Huel Jewel, near St. Die. It has been hitherto considered as a variety of grey copper ore (fahlerz); but its specific gravity is inferior, its hardness greater, and it has never been observed crystallized in tetrahedrons.

Its colour varies from lead-grey to iron-black.

It is usually crystallized in rhomboidal dodecahedrons, either perfect, or variously modified. Some of the modifications are figured by Mr. William Phillips, in the Quarterly Journal, vii. 97.

Crystals externally sometimes tin-white and splendent; sometimes lead-grey, and glistening; sometimes iron-black, and dull.

Lustre of the fragments from shining to glistening: metallic.

Fracture imperfectly foliated, and uneven, with the appearance of natural joints parallel to the faces of the rhomboidal dodecahedron.

Harder than vitreous copper or grey copper ore, both of which it scratches.

Brittle. Specific gravity 4·375. Powder reddish-grey.

Before the blow-pipe on charcoal, it burns first with a blue flame, and slight decrepitation; to which succeed copious arsenical vapours, leaving a greyish-black scoria which affects the magnetic needle.

Its constituents, according to the analysis of Mr. Richard Phillips, are as follows:

Silica	5·00
Iron	9·26
Copper	45·32
Sulphur	28·74
Arsenic	11·84
	100·16

—(See Quarterly Journal, vii. 95.)

I have a single specimen of this ore, which I brought from Cornwall (I think from the united mines near Redruth). I considered it as probably a variety of grey-copper ore, though it differed from it materially in lustre, colour, and hardness; but from Mr. Phillips's description, there seems no reason to doubt that it constitutes a peculiar species. As to the composition of the sulphuretted ores of copper, the subject is at present involved in impenetrable obscurity, in which it must remain till some chemist be fortunate enough to meet with specimens of each species perfectly free from all foreign matter.

II. NEW ANALYSES OF MINERALS.

1. *Tourmaline.*—In the number of Gilbert's Annalen der Physik for April, 1818, Lampadius announces that he and Breit-

haupt had discovered boracic acid in the tourmaline. He promises to give the quantitative results, and the mode of analysis, in a future paper, which I have not yet seen. This notice attracted the attention of M. Vogel, who actually succeeded, as he informs us (Journ. de Pharmacie, 1818, p. 338) in extracting a quantity of boracic acid from the black tourmaline of the Upper Palatinate (see an abstract of his paper in the *Annals of Philosophy*, xii. 314); but it still remains doubtful whether this acid exists in every variety of tourmaline; at least Prof. Gmelin, of Tubingen, who has devoted much of his time to the analysis of minerals, and who informed me that he had repeatedly analyzed the tourmaline in Berzelius's laboratory at Stockholm, and had always experienced as great a loss as that which had been sustained by Bucholz in the analyses which he had made of the tourmaline at the request of Prof. Bernhardi. Prof. Gmelin has again subjected the same mineral to a new analysis since the discovery announced by Lampadius, and verified by Vogel, without being able to detect the presence of boracic acid; but we must suspend our judgment till the modes of analysis followed by Lampadius and by Gmelin have been laid before the chemical world.

2. *Axinite.*—Vogel has announced the presence of boracic acid likewise in axinite. I should think it rather surprising that this acid should have been overlooked by Klaproth and Vauquelin, at least if it occur in considerable quantity, in a mineral which contains, according to their estimate, above the sixth of its weight of lime. If such oversights have been committed by two of the most expert analysts of the age, it is impossible not to conclude that the whole labour of analyzing the mineral kingdom remains still to be undertaken.

3. *Tantalite, or Columbite.*—This ore, formerly so scarce, has been observed first in Finland, in small grains disseminated in granite; and more lately at Bodenmais, in Germany, crystallized in pretty large four-sided prisms; at least this is the form of a specimen in my possession, for which I am indebted to the kindness of Mr. Heuland. Tantalite has been analyzed with great care by Berzelius, and more lately by Vogel. The following table exhibits the constituents as deduced from the analysis of each. The reader will bear in mind that the tantalite of Berzelius was from Finland, while that of Vogel was from Bodenmais.

	Berzelius.	Vogel.
Oxide of tantalum	83·2	75
Protoxide of iron	7·2	17
Protoxide of manganese	7·4	5
Oxide of tin	0·6	1
	98·4	98

4. *Petalite.*—This mineral, according to the mean of several careful analyses by Arvedson, is composed as follows:

Silica....................	79·212
Alumina	17·225
Lithina.	5·761
	102·198

He considers it as composed of an atom of the sexsilicate of lithina and three atoms of trisilicate of alumina. Hence its symbol will be $LS^6 + 3AS^3$.

5. *Spodumene, or Triphane.*—This mineral had been examined by Vauquelin, who had found it to contain 10 per cent. of potash. Hisinger and Berzelius analyzed it afterwards without detecting any alkaline ingredient whatever. This want of coincidence induced Arvedson to resume the examination of the mineral anew. The result of his experiments gave the constituents as follows:

Silica	66·40
Alumina.	25·30
Lithina	8·85
Oxide of iron.	1·45
Volatile matter	0·45
	102·45

Vogel's analysis of the Tyrolese spodumene, if we suppose that the alkali which he denominates potash was really lithina, does not differ very materially from the result obtained by Arvedson. It is as follows:

Silica	63·50
Alumina.	23·50
Lime	1·75
Potash	6·00
Oxide of iron.	2·50
Water.	2·00
Manganese	Trace
	99·25

(See *Annals of Philosophy*, xii. 392.)

6. *Green Tourmaline, called Crystallized Lepidolite.*—This mineral resembles the tourmaline, but is much softer; being easily scratched by a knife. Though it has been considered as a crystallized lepidolite, there can be no doubt, both from the shape of its crystals and from the result of the analysis of Arvedson, that it is merely a variety of tourmaline; the constituents which he found being nearly similar to those found by Klaproth and Vauquelin in the rubellite, Arvedson's analysis gave the constituents as follows:

Silica	40·3
Alumina	40·5
Lithina	4·3
Oxide of iron	4·85
Oxide of manganese	1·5
Boracic acid	1·1
Volatile matters	3·6
	96·15

7. *Foliated Pyrope, from Greenland.*—This mineral has a deep blood colour. Its lustre is not adamantine, like that of the pyrope, but common. It is composed of scaly distinct concretions. It is softer than pyrope. Its specific gravity is 3·634. According to the analysis of Pfaff, its constituents are as follows:

Silica	41·82
Oxide of iron	32·42
Alumina	17·82
Magnesia	4·90
Oxide of manganese	3·12
Lime	0·80
	100·88

This approaches to Klaproth's analysis of the pyrope. (Schweigger's Journal, xxi. 236.)

8. *Rutilite, from Arendahl.*—This mineral has a dark hair brown colour, passing into blackish brown.

It is always crystallized; but so confusedly that Professor Pfaff was unable to make out the form, though he thinks that it approaches most to a four-sided prism.

The external surface is dull or slightly glimmering. The lustre of the longitudinal fracture is glistening, that of the cross fracture shining, and the kind of lustre approaches that of the diamond.

The principal fracture is foliated with a two-fold cleavage meeting under angles of 74° and 106°. The cross fracture is small conchoidal.

It is composed of thick scaly distinct concretions.

The fragments are quadrangular and sharp edged.

It is transducent at the edges.

It scratches glass, and even garnet.

It is brittle and easily frangible.

The colour of the powder is light brown.

Specific gravity 3·879.

Its constituents, according to the analysis of Professor Pfaff, are as follows:

Silica	38·02
Protoxide of iron	34·00
Alumina	13·00
Oxide of titanium	7·00
Protoxide of manganese	5·15
Lime	1·40
Magnesia	0·60
	99·07

Professor Pfaff has observed that there is a striking resemblance between zirconia and the oxide of titanium. To prove this he has drawn up the following table:

(1.) Zirconia and oxide of titanium are both insoluble in caustic alkalies.

(2.) Both are somewhat soluble in carbonates of potash and soda.

(3.) The solution of zirconia in muriatic acid, when heated to a certain temperature, becomes milk white, and runs in some measure into a jelly, especially if it has been concentrated to a certain point by evaporation. The muriatic solution of oxide of titanium exhibits the same appearances.

(4.) From the muriatic solution of zirconia, oxalic acid throws down a white precipitate, which is again re-dissolved by an excess of the acid. This is the case also with the solution of oxide of titanium.

(5.) Zirconia and oxide of titanium are precipitated from their acid solutions by the neutral succinates and benzoates in copious white bulky flocks, which are again readily dissolved by the addition of succinic acid.

(6.) Tartaric acid, or tartrate of potash, occasions a precipitate when dropt into the solution either of zirconia or oxide of titanium.

(7.) Malic acid produces, in both solutions, a copious white precipitate.

(8.) Prussiate of potash throws down a *green* precipitate in the common solution of oxide of titanium; which, by a certain increase in the oxidation of the titanium, becomes almost quite *blue*. From a moderately neutral muriatic solution of zirconia prussiate of potash throws down a *greenish blue* precipitate, which, on the addition of muriatic acid, becomes more blue; but, after a certain interval of time, changes into celadon green. The liquid above both precipitates remains of the same green colour.

(9.) Hydrosulphuret of ammonia produces, in the muriatic acid solution of oxide of titanium, a dark olive or blackish green precipitate in very loose flocks. This precipitate may be washed without any loss of colour; but when exposed to sunshine it becomes quite white. The same phenomena take place when hydrosulphuret of ammonia is dropped into a solution of zirconia, and the precipitate undergoes the same change of colour when exposed to the solar rays.

(10.) The only re-agent which acts in a strikingly different manner upon solutions of oxide of titanium and of zirconia is the tincture of nutgalls. In the common solution of oxide of titanium it throws down a *reddish brown* precipitate, whereas in the solution of zirconia it occasions a deposition of *yellow flocks.* The addition of ammonia renders the colour more inclining to brownish red, and makes the precipitate more abundant.

(11.) Both the solution of oxide of titantium, and of zirconia, have an astringent taste.

It is obvious from this detail of particulars, that if zirconia and oxide of titanium be two distinct substances, as is believed at present, we are still ignorant of a method of separating them from each other.—(Schweigger's Journal, xxi. 240.)

This historical sketch has extended already to so great a length, that I must pass over the notice of the new analyses of various minerals which have been inserted in the twelfth and present volumes of the *Annals of Philosophy.* I refer the reader to *Annals of Philosophy,* xii. 388, 465, 468; and xiii. 65, 141, 144, 232, 310.

III. CRYSTALLINE FORM OF CINNABAR.

This mineral, which is almost the only one of mercury, occurs in great abundance, but seldom in crystals. Hence its crystalline form had not yet been determined with accuracy. Haüy, when he published his Mineralogy, had seen only two crystals, and he was led from them to suspect that the primitive form was a regular six-sided prism. M. le Chevalier de Parga has lately sent him a set of very complete crystals of this mineral from the mine of Almaden, in Spain, which has enabled him to determine the primitive form, and the laws of crystallization of this mineral, with all the requisite precision. He has accordingly published a memoir on the subject which will be duly appreciated by mineralogists. As it is scarcely possible to make his deductions intelligible, without the assistance of figures, I think it will be better to insert the memoir entire in a future number of the Annals. It may be sufficient to observe in this place, that the primitive form of the crystals of cinnabar, according to Haüy, is an acute rhomboid, the smallest incidences of the faces of which are 71° 48′, and the greatest 108° 12′. The ratio between the demidiagonals of each rhomb is $\sqrt{3}$ to $\sqrt{8}$. (Ann. de Chim. et Phys. viii. 64.)

IV. ON THE CAUSES OF THE DIFFERENT CRYSTALLINE FORMS OF MINERALS.

The great variety of forms which the same mineral species is known to assume, has drawn much of the attention, and occasioned the most laborious part of the investigations of mineralogists. The known forms of calcareous spar exceed 600; and perhaps those of iron pyrites and of some other species, if they were fully examined, would not be found much fewer. Leblanc was the first of the modern chemists that attempted to account for this diversity; but the progress which he made was inconsiderable. The subject has been lately taken up by M. Beudant, who has published a most interesting and elaborate paper on the subject. I regret that I am prevented, by want of room, from laying the substance of his researches before the reader. I can do no more than merely state the general results which he obtained.

1. The state of the atmosphere, the greater or less rapidity of evaporation, the form of the vessel, its nature, the quantity of liquid, the state of its concentration, seem to have no effect whatever upon the crystalline forms which salts assume; they merely influence their beauty and size.

2. When the atmosphere is moist, the salts have a tendency to form crystalline vegetations on the edges of the vessel.

3. Very dilute solutions, excluded from the air and prevented from evaporating, may yield crystals after a longer or shorter interval of time. But this is particularly the case with those salts which have but little solubility.

4. The nature of the vessels, by exercising different attractions on the salts, occasions the crystals to deposit themselves more or less quickly, and to accumulate in different ways in different parts of the solution. If the vessels are covered with a coat of grease, the crystallization takes place only at the surface.

5. The position in which the crystals are deposited in the midst of a liquid mass, has no other influence than that of producing more or less extension of the crystal in one direction, rather than another. The bounding faces are always of the usual number, and in the usual position.

6. The temperature and electrical state seem to have no influence on the forms of crystals; excepting that at high temperatures crystallization is very irregular, and the saline masses produced are very fragile.

7. Substances in suspension, *almost* permanent in a saline solution, have no effect in varying the crystalline form. These substances are often deposited in the crystal in concentric layers.

8. The crystallization of a salt cannot take place in the midst of a deposit of foreign matters in very fine and incoherent particles, unless the deposit be covered to a certain height by the liquid. Crystals, formed in these circumstances, always contain a portion of the foreign matters which are found disseminated more or less regularly in their mass, and never deposited in concentric layers. When the solution is not much con-

centrated, the crystals are always of a simpler form and more regular than when they are crystallized in a pure liquid. When the solution is very concentrated, isolated crystals are formed in it, whose faces are crossed like the hopper of a mill.

9. The crystallization of a salt may take place in the midst of a gelatinous mass without the necessity of any supernatant liquid. In that case the crystals contain none of the foreign matter, and undergo no change of form; but they are almost always isolated and remarkably regular and complete in all their parts.

10. When several salts are in solution in the same liquid, it would appear that they are capable of mutually affecting one another's crystallization, even when they are not susceptible of uniting or of acting chemically upon each other. Thus common salt takes the form of a cubo-octahedron when it crystallizes in the midst of a solution of borax, or still better of boracic acid.

11. The forms which the same salt is capable of assuming, vary according to the nature of the liquid from which it is precipitated. Thus alum assumes the cubo-octahedral form when it crystallizes in nitric acid, and the cubo-icosahedral form when it crystallizes in muriatic acid.

12. Whenever several salts are capable of mixing chemically, that is to say, of uniting without entering into a definite combination, that salt, whose system of crystallization predominates, always assumes particular forms which differ from those which it adopts when it is pure. The different salts present likewise, in general, different forms in the same system of crystallization, according as they contain more or less of acid; and the double salts according as one or other of the component salts exist in more or less quantity.

13. The chemical action which tends to determine a particular form, by altering the composition of a salt, produces different effects according to its energy, and often gives occasion at once to several varieties of crystals. Thus the action of an insoluble carbonate upon alum determines in the same solution octahedral crystals, cubo-octahedral crystals, cubic crystals, and an incrystallizable matter which contains still less acid than the preceding.

14. When simple crystals of different forms belonging to the same salt are dissolved together in the same liquid, two different things may happen. If the crystallization takes place slowly, the crystals are deposited in succession and separately; but if the crystallization be rapid, a single mixed compound is formed, exhibiting crystals partaking at once of all the different simple forms. Thus octahedral and cubic crystals of alum may unite and constitute cubo-octahedral crystals.

15. Crystals of complex form may be sometimes decomposed

into several simple forms by different solutions and successive slow crystallizations. Thus cubo-octo-dodecahedral alum yielded separate octahedrons, cubes, and cubo-dodecahedrons.

16. Crystals of a certain form being put into a solution of the same substance, which gives naturally a different form, increase by additions according to this new form.—(See Ann. de Chim. et Phys. viii. 5.)

V. ELECTRICITY OF MINERALS.

M. Haüy, assisted by M. Delafosse, has made a very elaborate set of experiments to determine the electrical state of the different species of minerals. I can here give nothing more than a tabular view of the results which they obtained. This, indeed, is sufficient, as the mode of trying the electricity of minerals has been long familiar to mineralogists.

CLASS I.

Substances transparent and colourless in their perfect state. Their colour, when they have any, depends upon an accidental principle. They are capable of insulating, and acquire, when rubbed, the vitreous or positive electricity.

ORDER I.

Electrical by Heat.

Borate of magnesia,
Silico-fluate of alumina,
Axinite,
Tourmaline,
Mesotype,
Prehnite,
Oxide of zinc,
Titane siliceo-calcaire.

ORDER II.

Nonelectric by Heat.

A. *Saline.*

Calcareous spar,
Ditto, containing magnesia in laminæ, from St. Gothard,
Arragonite,
Phosphate of lime (asparagus stone),
Fluate of lime,
Sulphate of lime,
Anhydrous ditto,
Sulphate of barytes,
Carbonate of barytes,
Sulphate of strontian,
Carbonate of strontian,
Sulphate of magnesia,
Silico-borate of lime,
Nitrate of potash,
Sulphate of potash,
Common salt,
Glauberite.

B. *Earthy.*

Quartz,
Zircon,
Corundum
Cymophane,
Spinel,
Emerald,
Euclase,
Dichroite,

B. *Earthy* (*continued*).

Garnet,
Essonite,
Idocrase,
Felspar,
Apophyllite,
Actinolite and tremolite,
Diopside,
Epidote,
Stilbite,
Analcime,
Nepheline,
Disthene or cyanite,
Mica,
Macle.

C. *Combustible.*

Diamond,

D. *Metallic.*

Carbonate of lead,
Sulphate of lead,
Tungstate of lime,
Carbonate of zinc,
Oxide of tin.

The following species are placed here merely from analogy:

Carbonate of magnesia,
Borax,
Sal-ammoniac,
Alum,
Cryolite,
Wavellite,
Spodumene,
Petalite,
Granatite,
Hyperstene,
Wernerite,
Scapolite,
Diallage,
Anthophyllite,
Lemonite,
Sodalite,
Chabasite,
Harmotome,
Pinite,
Dipyre,
Asbestos.

Appendix.

Substances exhibiting resinous or negative electricity joined to an unctuous feel. They are capable of insulating when transparent and colourless.

Foliated talc,
Granular talc?
Agalmatolite?

CLASS II.

Substances having a peculiar colour depending on their nature, capable of insulating in what state soever they are, and acquiring, when rubbed, resinous or negative electricity. Anthracite alone must be insulated before it can be excited.

Sulphur,
Bitumen,
  a. glutinous,
  b. solid,
  c. elastic,
Retinasphalt,
Amber,
Mellite,
Anthracite.

CLASS III.

Substances essentially opaque, possessing the metallic lustre, or acquiring it when polished, conductors, and acquiring when insulated, some of them vitreous, and others resinous electricity.

ORDER I.

Vitreous Electrics.

Silver,
Native silver,
Silver coin,
Lead,
Copper,
Native copper,
Copper coin,
Zinc,
Brass,
Native bismuth,
Argental mercury.

ORDER II.

Resinous Electrics.

A. *Having naturally the Metallic Lustre.*

1. *Simple Species.*

Platinum,
Native platina,
Palladium,
Gold,
Native gold,
Gold coin,
Nickel,
Native iron,
Forged iron,
Tin,
Foil of looking-glasses,
Native arsenic,
Antimony,
Native antimony,
Auro-plumbiferous tellurium.

2. *Combinations of Two Metals.*

Antimonial silver,
Arsenical nickel,
Arsenical iron.

3. *Oxides.*

Protoxide of iron,
Peroxide of manganese.

4 *Metals united to Combustibles.*

Sulphuret of silver,
Sulphuret of lead,
Copper pyrites,
Grey copper ore,
Sulphuret of copper,
Graphite,
Sulphuret of iron,
White sulphuret of iron,
Magnetic sulphuret of iron,
Sulphuret of tin,
——— of bismuth,
——— of manganese,
——— of antimony,
——— of molybdenum.

5. *Metalline Salts.*

Chromate of iron.

B. *Exhibiting only a tendency to the Metallic Lustre, which they acquire sensibly when polished.*

Peroxide of iron,
Black oxide of cobalt,
Protoxide of uranium,
Wolfram,
Yenite,
Oxide of tantalum,
Yttro-tantalite,
Black oxidized cerium.

CLASS IV.

Substances having a colour, depending on their nature, susceptible of transparency in their perfect state. The property of insulating is limited to those varieties which approach that state.

ORDER I.

Susceptible of giving by reflexion the metallic lustre, and by reflection and refraction at once a colour more or less lively. The difference depends on the polish of the surface. They all acquire resinous electricity by friction.

Colour red by Transmission.

Sulphuretted antimonious silver,
Sulphuret of mercury,
Protoxide of copper,
Oligiste iron ore,
Sulphuret of arsenic,
Oxide of titanium.

Colour blue by Transmission.

Titane anatase.

ORDER II.

Destitute of the Metallic Lustre. Almost all acquire Resinous Electricity when rubbed.

Muriate of mercury,
Chromate of lead,
Phosphate of lead,
Molybdate of lead,
Green carbonate of copper,
Blue carbonate of copper,
Arseniate of copper,
Dioptase copper,
Phosphate of copper,
Hydrate of copper,
Sulphate of copper,
Phosphate of iron,
Arseniate of iron,
Sulphate of iron,
Sulphuret of zinc,
Arseniate of cobalt,
Oxide of uranium.

II. GEOGNOSY.

This historical sketch has been insensibly carried to such a length, that I am deprived of the power of entering into those geological details which the popularity of the science, and the zeal with which it has been cultivated in Great Britain, and in some other countries, would have rendered both amusing and instructive. I regret this preclusion the less, because the most important facts which have come to my knowledge, either have or will make their appearance in the transactions of the different geological societies which have been of late years established in Great Britain. I shall take care to insert a regular analysis of the different volumes published by these societies into the *Annals of Philosophy* soon after they have made their appearance.

There is only one publication belonging to *Geology*, strictly so called, which has made its appearance since my last historical sketch was drawn up. I allude to a work, intitled, "Facts and Observations towards forming a new Theory of the Earth, by William Knight, LL. D. Professor of Natural Philosophy in the Institution of Belfast." I abstain the more willingly from entering into any discussion respecting the theory of the earth, which the author has advanced, and which he has supported with much zeal and ingenuity, because the world in general seems now sensible of the unprofitable nature of such speculations. Even Professor Jameson, whose zeal burned for so many years with such furious ardour, that to call in question a Wernerian opinion, or to hesitate about the propriety of a Wernerian arrangement, was considered by him as a crime of the deepest die, and worthy of the severest treatment; even he has become sufficiently cool, has ventured to call in question some of the most material parts of his master's geognosy; and if he exercise his own judgment without fetters for a few years longer, I venture to predict that he will not be a Wernerian at all. Even the Huttonians, those Calvinists of the science of geology, whose theory was so complete and so beautiful, if we took its foundation for granted, and were complaisant enough to overlook its inconsistency with the phenomena of nature—even they have become a great deal more tolerant; they no longer hurl their anathemas and their interdicts against their antagonists; they no longer affirm that mineralogy and geology are unconnected sciences, and that we may become profound geologists without any knowledge whatever of rocks or of minerals. On the contrary, they have exercised their industry with laudable zeal, and not only favoured us with descriptions of tracts of country themselves, but encouraged others to undertake similar tasks. Geologists in general seem now satisfied that the true object of their science is to acquire an accurate knowledge of the structure of the earth; that this knowledge can be acquired only by patient observation; that at present our knowledge of that structure is very incomplete; and that till the position of all the different strata over the whole surface of the earth be accurately ascertained, it would be a waste of time to speculate upon the original formation of these strata, or the changes which they have undergone since their original creation. Dr. Knight is a gentleman of amiable manners, of excellent abilities, and indefatigable industry. He would much more effectually promote the interests of his favourite science by an accurate description of the numerous parts of Scotland and Ireland, whose structure he has ascertained, than by the most ingenious speculations about the origin of the earth. The splendour of such speculations is too apt to have irresistible attractions for a young and generous mind just starting in the arena, and eager to attract the attention of his fellows. But the fate of the numerous list of preceding writers in this tempting career, and the fate obviously impending over even the latest and best qualified adventurers, ought, I think, to be a warning. Who at present ranks the geological speculations of Kirwan, Bertrand, or Lametherie, much higher than those of Woodward or Buffon? And the impending fate of Hutton, and even of Werner, is obvious and irresistible. Facts are eternal, speculations are palaces of ice glittering like gold and jewels, and built apparently of the most solid materials; but melting away before the rays of the sun, without leaving even a trace behind them.

ANNALS

OF

PHILOSOPHY.

JANUARY, 1819.

ARTICLE I.

Observations on new Combinations of Oxygen and Acids.

By M. Thenard.*

I OBTAINED these new combinations by treating the peroxide of barium with acids. Most of them are very remarkable, and deserving the attention of chemists.

The first that I observed is the combination of nitric acid and oxygen. When the peroxide of barium, prepared by saturating barytes with oxygen, is moistened, it falls to powder without much increase of temperature. If in this state it be mixed with seven or eight times its weight of water, and dilute nitric acid be gradually poured upon it, it dissolves gradually by agitation without the evolution of any gas. The solution is neutral, or has no action on turnsol or turmeric. When we add to this solution the requisite quantity of sulphuric acid, a copious precipitate of sulphate of barytes falls, and the filtered liquor is merely water holding in solution oxygenized nitric acid.

This acid is liquid and colourless, it reddens strongly turnsol, and resembles in almost all its properties nitric acid.

When heated, it immediately begins to discharge oxygen; but its decomposition is never complete unless it be kept boiling for some time. It follows from this, that it would be difficult to concentrate it by heat without altering it. The only method which succeeded with me was to place it in a capsule under the receiver of an air-pump along with another capsule full of lime, to exhaust the receiver till the barometer gauge stands 10 or 12 centimetres below the common barometer. By this means I obtained an acid sufficiently concentrated to give out 11 times its bulk of oxygen gas; while in its first state it gave out only $1\frac{1}{2}$ times its bulk of that gas.

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* Translated from the Ann. de Chim. et Phys. viii. 306. (July, 1818.)

This acid combines very well with barytes, potash, soda, ammonia, and neutralizes them; but I am afraid that it will scarcely be possible to crystallize the salts thus formed. When heated ever so little, the acid is decomposed, and gives out oxygen. They are decomposed likewise, at least this is the case with the oxygenized nitrate of barytes, when left to spontaneous evaporation. The decomposition takes place at the instant of crystallization. They are decomposed likewise when placed under an exhausted receiver. They have this last property in common with the solutions of the alkaline bicarbonates, which, when placed in an exhausted receiver, boil violently, and are reduced to the state of carbonates. The oxygenized nitrate, when changed into nitrates, do not alter the state of their neutralization.

Thus we see that oxygenized nitric acid, when united with bases, instead of becoming more stable, acquires, on the contrary, the property of abandoning its oxygen with greater facility. This is so true, that if into a neutral and concentrated solution of oxygenized nitrate of potash we pour a concentrated solution of potash, a brisk effervescence takes place, and oxygen is disengaged. The potash acts doubtless upon the nitrate, properly so called. Thus the bases act relatively to oxygenized nitric acid as the ordinary acids relatively to certain peroxides; sulphuric acid, for example, on the black oxide of manganese.

I have not neglected to put oxygenized nitric acid in contact with the metals. I found that it did not act on gold; that it dissolved very well those metals which nitric acid is capable of dissolving; and that this solution in general took place without the disengagement of gas, and with the production of heat. However, in some cases, there is a little oxygen disengaged at first. This happens when the action is too violent, as is the case when oxygenized nitric acid concentrated so as to contain 15 times its volume of oxygen is poured upon zinc.

One of the most important questions was to know how much oxygen oxygenized nitric acid contained. In order to ascertain the quantity, I began by analyzing the deutoxide of barium. I heated a certain quantity of barytes with an excess of oxygen in a small curved tube standing over mercury. This base, to pass to the state of a peroxide, absorbed almost as much oxygen as it contained; but having ascertained that barytes extracted from the nitrate always contains a little peroxide, I conclude that in the peroxide, the quantity is double that which exists in the protoxide; but in the neutral nitrates, the quantity of oxygen of the acid is to the quantity of oxygen of the oxide as five to one.* Consequently in oxygenized nitric acid, the azote will be to the oxygen in volume as one to three. I reason here on the supposition that the acid is pure; that is to say, is not a mixture of nitric and oxygenized nitric acid.

Phosphoric, arsenic, and probably boracic acid, are capable, like nitric acid, of uniting with oxygen: they retain it much more strongly. This is the case also with the oxygenized arseniates and phosphates; so that I am in hopes of being able to obtain these salts in a solid state.

I have not yet been able to procure oxygenized sulphuric acid. All the attempts which I have hitherto made have been indecisive.

My experiments on acetic acid have been much more conclusive. This acid dissolves the deutoxide of barium with almost as great facility as nitric acid does. No effervescence takes place, and we obtain by the process described above, an acid, which, being saturated with potash and heated, allows a great quantity of oxygen gas to escape. There is disengaged at the same time a notable quantity of carbonic acid gas. This shows that the oxygen, when assisted by heat, unites in part with the carbon, and doubtless likewise with the hydrogen of the acid.

Guided by the preceding experiments, I examined likewise the action of liquid muriatic acid on the deutoxide of barium. I expected that the result would be water, chlorine, and muriate of barytes; but the result was quite different. I obtained oxygenized muriatic acid, which I separated by means of sulphuric acid. This fact appeared to me so extraordinary, that I multiplied experiments in order to demonstrate it. One of the most decisive of these is the following:—I took a fragment of barytes, which, in order to pass to the state of deutoxide, had absorbed 12·41 centilitres of oxygen gas; I mixed it with water, and then dissolved it in diluted muriatic acid. After this, I precipitated all the barytes by means of sulphuric acid. The filtered liquid was neither precipitable by sulphuric acid, nor by nitrate of barytes. In this state I saturated it with potash, and heated it gradually till it boiled. I obtained very nearly the original volume of oxygen absorbed at first by the base. If I add that oxygenized muriatic acid leaves no residue when evaporated; that the barytes after its oxygenation requires for passing to the state of a neutral muriate the same quantity of acid as before the oxygenation; that the muriate formed exactly resembles common muriate, the existence of oxygenized muriatic acid will not, I conceive, admit of doubt.

I have obtained it only at the degree of concentration, in which it contains four times its volume of oxygen. It is a very acid, colourless liquid, almost destitute of smell, and powerfully reddens tincture of turnsole. When raised to the boiling temperature, it is decomposed, and converted into oxygen and muriatic acid. When saturated with barytes, potash, or ammonia, it is decomposed still more readily, allowing a quantity of oxygen to escape. It dissolves zinc without effervescence. It does not act upon gold at the ordinary temperature, at least in the space of a few minutes. Its action on the oxide of silver is curious. These two bodies occasion as lively an effervescence as if an acid were poured upon a carbonate. The reason is, that water and a chloride being formed by the reaction of the oxide and the muriatic acid on each other, the oxygen united with the acid is suddenly disengaged and assumes the gaseous form.*

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* This law holds only when the bases are protoxides. Many bases may be mentioned, as peroxide of mercury, peroxide of cerium, in which the proportions are different.—T.

The property which oxygenized muriatic acid has of being decomposed by oxide of silver, so that the oxygen becomes free, will probably put it in our power to form several other oxygenized acids with facility. Thus with oxygenized muriatic acid and a solution of fluate of silver, we may expect to obtain oxygenized fluoric acid.

In oxygenized muriatic acid, the hydrogen and oxygen are in the proportions requisite for forming water.

Such are the principal results which I have hitherto observed. They make us acquainted with a new class of bodies, which will, perhaps, be numerous in species. We must find them out, ascertain their properties, examine the different circumstances in which they are susceptible of being formed, we must see whether other bodies as well as acids be not capable of combining with oxygen. Thus a laborious series of experiments is chalked out, the parts of which I propose to present to the Academy in proportion as I ascertain them.

Since these observations were read, I have satisfied myself that by the process pointed out for obtaining oxygenized fluoric acid, not only this acid may be obtained, but likewise oxygenized sulphuric acid. Indeed it will be easy to obtain in that way all the acids susceptible of being oxygenized.

Oxygenized fluoric acid does not let go its oxygen at a boiling temperature; but oxygenized sulphuric acid lets it go easily.

I have ascertained likewise that oxygenized nitric and muriatic acids may be combined with new doses of oxygen. Probably the other acids are in the same case. To obtain these new compounds, it is sufficient to treat the oxygenized acid with the deutoxide of barium, as above described; for example, to superoxygenize oxygenized muriatic acid, this acid is saturated with deutoxide of barium. The barytes is precipitated by sulphuric acid, and the liquor is decanted off. It will be found to contain all the oxygen coming from the two portions of deutoxide of barium on which the operation was performed.

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* The discovery of this new compound seems to set the controversy respecting the nature of *chlorine* at rest. Prof. Berzelius, and those other gentlemen who maintain the old doctrine, will now be able to satisfy themselves that chlorine and oxygenized muriatic acid are two distinct substances.—T.

It is worthy of remark, that the same acid may be oxygenized several times repeatedly by the same process. I have oxygenized it as often as seven times.

Do these sorts of combinations take place in definite or indefinite proportions? This must be ascertained by future experiments.

Be this as it may, when an excess of barytes water is poured into oxygenized nitric or muriatic acid, or into these acids superoxygenized, a crystalline precipitate of deutoxide of barium falls. This precipitate is very abundant; it has the form of pearly scales, and is but little soluble in water. This liquid, at the temperature of 50°, decomposes it, and converts it into oxygen gas and barytes, or protoxide of barium.

Strontian and lime are susceptible of being superoxygenized, as well as barytes, by the superoxygenized acids. The hydrate of deutoxide of strontian resembles considerably that of barium: that of lime is in finer plates.

Probably by the same method I shall be able to oxygenize the earths, or, at least, some of them; and I shall be able to superoxydize a great many metallic oxides. To accomplish this, I propose to put an excess of base with the acid, or to dissolve the base in the acid, and then to precipitate it by potash; or I shall put the oxygenized muriates in contact with oxide of silver, which, seizing on the muriatic acid, will, in that way, favour the combination of the oxygen with the oxide which it is wished to superoxygenize.

ARTICLE II.

New Experiments on the Oxygenized Acids and Oxides.[*]

By M. L. J. Thenard.

I ANNOUNCED in my preceding observations, that muriatic, nitric acids, &c. were susceptible of being oxygenated several times. It was of importance to be able to determine the quantity of oxygen which they were capable of taking up. This I have done with regard to muriatic acid, as I shall state briefly. I took liquid muriatic acid of such a degree of strength that when combined with barytes, a solution was produced, which, when slightly evaporated, deposited crystals of muriate of barytes. I saturated this acid with deutoxide of barium reduced into a soft paste by water and trituration. I then precipitated the barytes from the liquid by adding the requisite quantity of sulphuric acid. I then took the oxygenized muriatic acid and treated it with deutoxide of barium and sulphuric acid to oxygenize it anew. In this way I charged it with oxygen as often as 15 times. This process is conducted the first five or six times without the evolution of oxygen gas; especially if the muriatic acid be not completely saturated, and if the muriate be poured into the sulphuric acid; but beyond that point it is difficult not to lose a little oxygen. However, the greatest part of this gas remains united to the acid. In this way I obtained an acid which contained 32 times its volume of oxygen at the temperature of 68° Fahr. and under a pressure of 29·922 inches of mercury; and only 4½ times its volume of muriatic acid; that is to say, that the volume of oxygen being seven, that of the muriatic acid was only one.*

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[*] Translated from the *Ann. de Chim. et Phys.* ix. 51. (Sept. 1818.)

Although the oxygenized muriatic acid, prepared in the way just described, contains a great quantity of oxygen, it is not yet saturated with it, being still capable of receiving a new portion. But to make it absorb the gas with facility, we must adopt a new method. This method consists in putting the oxygenized muriatic acid in contact with the sulphate of silver. There is immediately formed insoluble chloride of silver and oxygenized sulphuric acid, which is very soluble. When this last is separated by the filter, muriatic acid is added, but in smaller quantity than what existed in the oxygenized muriatic acid employed at first. A quantity of barytes, just sufficient to precipitate the sulphuric acid, is then added. Instantly the oxygen leaving the sulphuric acid to unite with the muriatic acid brings that acid to the highest point of oxygenation. Thus we see that we can transfer the whole of the oxygen from one of these acids to the other; and on a little reflection, it will be evident that to obtain sulphuric acid in the highest degree of oxygenation, it will be merely necessary to pour barytes water into oxygenized sulphuric acid so as to precipitate only a part of the acid. All these operations, with a little practice, may be performed without the least difficulty.

By combining the two methods just described, I can obtain oxygenized muriatic acid containing nearly 16 times as many volumes of oxygen as of muriatic acid.† It was so weak, that from one volume of acid I could only extract 3·63 volumes of oxygen gas under a pressure of 29·922 inches of mercury, and at the temperature of 65·3°.

Oxygenized muriatic acid exhibited several new phenomena to me, worthy of being related.

When recently prepared, it does not disengage any air bubbles when filtered; but soon after we perceive very small bubbles make their appearance at the bottom of the vessel, ascend, and burst at the surface of the liquid. This even happens when the acid is only once oxygenized. Suspecting that this slow decomposition might proceed from the action of light, I filled almost completely a small flagon with acid, and after corking it, turned it upside down, and placed it in a dark place. After some hours, an explosion took place. The acid contained more than 30 times its volume; yet when this same acid was put under the exhausted receiver of an air-pump, it allowed but a small quantity of the gas which it contained to be disengaged.

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* Such an acid must be composed of 1 atom muriatic acid and 28 atoms oxygen!—T.

† Such acid must be a compound of 1 atom muriatic acid and 64 atoms *oxygen!—T.*

Hitherto I had imagined that the whole of the oxygen was disengaged from the muriatic acid at a temperature below ebullition; but this is not the case. After boiling oxygenized muriatic acid for half an hour, I still found oxygen in it.

It is by means of the oxide of silver that we can demonstrate the presence of oxygen in oxygenized muriatic acid which has been boiled. Scarcely does it come in contact with it but oxygen is suddenly disengaged. This oxide enables us to determine with facility the quantity of oxygen contained in oxygenized muriatic acid. The analysis requires only a few minutes. Take a graduated glass tube, fill it almost entirely with mercury, pour into it a determinate volume of acid, fill the tube completely with mercury, and turn it upside down in the mercurial trough. Let up into the acid an excess of oxide of silver suspended in water. Immediately we see disengaged, and may read off on the tube, the quantity of oxygen contained in the acid. We can estimate the quantity of chlorine; and, of consequence, the muriatic acid, by decomposing a part of the acid itself by means of nitrate of silver.*

The disengagement of oxygen from the oxygenized muriatic acid is so rapid, that it would be dangerous to operate upon a weak acid, which contained 26 or 30 volumes of oxygen. The tube would probably escape from the hands of the operator, or would break. Accordingly nothing can equal the effervescence which takes place when we plunge a tube containing oxide of silver and agitate it in some grammes of the acid of which we have just spoken. As that acid is immediately destroyed, the oxygen is restored to its liberty, and escapes with violence, driving the liquid before it.

When the most oxygenized muriatic acid is poured upon the sulphate, the nitrate, or the fluate of silver, no effervescence takes place. All the oxygen unites with the acid of the salt, while the muriatic acid forms with the oxide of silver water and a chloride.

I have already made several attempts to ascertain if the oxygenized acids be capable of taking up so much the more oxygen the more real acid they contain; or whether the water by its quantity has not an influence on the greater or smaller oxygenizement of the acid. My essays have not yet enabled me to answer that question.

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* Having just observed that in this experiment there is a portion of the oxygen of the oxide of silver disengaged, it is obvious that we must take an account of this quantity to get an accurate *result. See next* paper.

I have attempted likewise, without any decisive success, to oxygenate magnesia and alumina; but I have succeeded in superoxygenating several other oxides; namely, oxide of zinc, oxide of copper, oxide of nickel. We should not succeed, or, at least, we should succeed very imperfectly, if we satisfied ourselves with adding oxygenized acid to the saline solutions of these three metals and precipitating the liquid by potash.

It is necessary to dissolve the oxides of these metals in oxygenized muriatic acid three or four times, and to decompose the oxygenized muriate by potash or soda, taking care to add but a small excess of these bodies. The preparation of superoxide of copper requires an additional precaution; namely, to put the deutoxide of copper into oxygenized muriatic acid in portions; so that the acid shall always be in excess. If the oxide predominates, the greater part of the oxygen is disengaged. In all cases the oxide is precipitated in a gelatinous mass, or in the state of a hydrate. That of zinc is yellowish; that of copper olive-green; and that of nickel, dirty, dark apple-green. The first two allow a portion of their oxygen to be disengaged at the ordinary temperature. When they are boiled in water, the disengagement is still more abundant; but they do not give out (especially the superoxide of zinc) all the oxygen which they have absorbed; for when we dissolve them afterwards in muriatic acid, and heat the liquid, we obtain a new quantity of gas. The oxide of nickel is decomposed likewise at the boiling temperature, and its decomposition begins even below that point. When treated with muriatic acid, it is dissolved like the oxides of zinc and copper, and is disoxygenated by heat without the evolution of chlorine. We may add likewise that these different oxygenated hydrates recover the colours which characterize the ordinary oxides after they have been boiled in water. Thus the hydrate of zinc passes from yellow to white, that of copper from olive-green to dark brown. M. Rothoff, a Swedish chemist, had already observed that the deutoxide of nickel is decomposed by desiccation.

These new hydrates resemble, as we see, those of barytes, strontian, and lime, and form a class analogous to that of the oxygenized acids. I shall probably discover more of them.

Article III.

*Fifth Series of Observations on the Oxygenized Acids and Oxides.** By M. Thenard. (Read to the Academy of Sciences Oct. 5, 1818.)

The facts, of which this series of observations consist, are so remarkable that they will probably excite some surprize in the most distinguished chemists. I shall state them as concisely as possible.

1. Oxygenized nitric and muriatic acids dissolve the hydrate of the deutoxide of mercury without effervescence; but if we afterwards pour an excess of alkali into the solution, much oxygen is disengaged, and the oxide of mercury, which reappears at first with a yellow colour, is speedily reduced.

2. This hydrate is reduced equally when placed in contact with the oxygenized nitrate, or muriate of potash. We see it pass from yellow to grey, while, at the same time, much oxygen is disengaged.

3. Oxide of gold extracted from the muriate by means of barytes, and containing a little of that base which gave it a greenish tint, was put, while in a gelatinous state, into oxygenized muriatic acid. A strong effervescence immediately took place, owing to the disengagement of oxygen. The oxide became purple, and soon after was completely reduced.

4. Oxygenized sulphuric, nitric, and phosphoric acids make the oxide of gold become at first purple, as well as oxygenized muriatic acid; but the oxide, instead of assuming afterwards the aspect of gold precipitated by the sulphate of iron, becomes dark brown. These experiments seem to tend to show the existence of a purple oxide of gold.

5. When oxygenized nitric acid is poured upon oxide of silver, a strong effervescence takes place, owing entirely to the disengagement of oxygen, as in the preceding cases. One portion of the oxide of silver is dissolved, the other is reduced at first, and then dissolves likewise, provided the quantity of acid be sufficient. The solution being completed, if we add potash to it by little and little, a new effervescence takes place, and a dark violet precipitate falls; at least, this is always the colour of the first deposite. This deposite is insoluble in ammonia, and, according to all appearance, is a protoxide of silver, similar to what an English chemist has observed while examining the action of ammonia on oxide of silver.

6. Oxygenized sulphuric and phosphoric acids likewise partially reduce the oxide of silver, occasioning a strong effervescence.

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* Translated from the Ann. de Chim. et Phys. ix. 94.

7. I have already spoken of the action of oxide of silver on oxygenized muriatic acid, and I stated that these bodies by their mutual action produced water, the disengagement of oxygen gas, and the chloride of silver. But this chloride is violet. Now violet chloride, in what manner soever it is produced, leaves always a metallic residue when treated with ammonia—a phenomenon, which Gay-Lussac observed respecting the white chloride rendered violet by the action of light. It follows from this, that when oxygenized muriatic acid is treated with oxide of silver, a small part of the oxygen disengaged comes from the oxide itself. Consequently to determine, by the process, which I pointed out in the last paper, the quantity of oxygen in oxygenized muriatic acid by means of oxide of silver, we must take an account of the oxygen proceeding from that portion of oxide. To do this we must make a second experiment, in which we collect the chloride of silver, produced and mixed with oxide of silver. This mixture is treated with ammonia, and we obtain as a residue the portion of the metal that had been reduced. The quantity of this residue makes us immediately acquainted with the quantity of oxygen wanted.

I shall remark, with respect to the violet chloride of silver, that it probably corresponds with the protoxide of silver.

8. As soon as we plunge a tube containing oxide of silver into a solution of oxygenized nitrate of potash, a violent effervescence takes place, the oxide is reduced, the silver precipitates, the whole oxygen of the oxygenized nitrate is disengaged at the same time with that of the oxide; and the solution, which contains merely common nitrate of potash, remains neutral, if it was so at first.

9. Oxide of silver produces the same effects upon oxygenized muriate of potash as upon the oxygenized nitrate.

10. If silver in a state of extreme division be put into the oxygenized nitrate, or muriate of potash, the whole oxygen of the salt is immediately disengaged. The silver is not attacked, and the salt remains neutral as before. The action is less lively (indeed much less lively) if the metal be in a less divided state. In all cases the action is less violent in the muriate than the nitrate.

11. Silver is not the only metal capable of separating the oxygen of the oxygenized nitrate and muriate of potash. Iron, zinc, copper, bismuth, lead, platinum, possess likewise this property. Iron and zinc are oxydized, and, at the same time, occasion the evolution of oxygen. The others are not sensibly oxydized. They were all employed in the state of filings.

I tried likewise the action of gold and of tin. These metals do not act sensibly on the neutral solutions, or, at most, only a few bubbles are disengaged at intervals.

12. Several oxides, besides those of silver and mercury, are capable of decomposing the oxygenized nitrate and muriate of

potash. I shall mention in particular the peroxide of manganese and that of lead. Only a small quantity of these oxides in powder is necessary to drive the whole of the oxygen from the saline solution. The effervescence is lively. I believe that the peroxide of manganese does not undergo any alteration. It is possible that the peroxide of lead may be reduced to a less degree of oxidation.

13. It is known that nitric acid has no action on the peroxide or manganese and of lead; but this is not the case with oxygenized nitric acid. It dissolves both of them with the greatest facility. The solution is accompanied by a great disengagement of oxygen gas. Potash produces in the manganese solution a black, flocky precipitate; and in that of lead, a brick coloured precipitate. This last is less oxydized than peroxide of lead; for when treated with nitric acid, we obtain nitrate of lead and a flea coloured residuum. At the instant of the addition of the potash there is a strong effervescence.

14. The oxygenized sulphates, phosphates, and fluates, exhibit the same phenomena with the oxide of silver, with silver, and probably with other bodies, as the oxygenized nitrate and muriate of potash. The greater number of the oxygenized alkaline salts possess the same properties as the oxygenized salts of potash.

What is the cause of the phenomena which we have just stated? This is a question which we must endeavour hereafter to resolve.

For this purpose, let us recall the phenomena which oxide of silver and silver exhibit with the neutral oxygenized nitrate of potash. Silver in a fine powder rapidly disengages the oxygen of this salt. It undergoes no alteration, and the oxygenized nitrate is reduced to the state of simple nitrate.

The oxide of silver disengages still more rapidly than silver the oxygen of the oxygenized nitrate. It is itself decomposed; it is reduced; the silver is totally precipitated; and we find in the liquid only common neutral nitrate of potash. Now in these decompositions the chemical action is evidently null. We must, therefore, ascribe them to a physical cause; but they depend neither upon heat nor upon light. Hence it follows, that they are probably owing to electricity. I shall endeavour to ascertain this point in a positive manner. I shall endeavour to ascertain likewise whether the cause, be it what it may, cannot be produced by the contact of two liquids and even of two gases. From this, perhaps, will be derived the explanation of a great variety of phenomena.

Article IV.

On the Phenomena of Sanguification, and on the Blood in general.

By W. Prout, M.D.

[Part of the following paper has already been laid before the public, under the title of an "Inquiry into the Origin and Properties of the Blood:" as, however, it was never completed, and as the work in which it appeared had a very limited circulation, the author has been induced to correct and republish the whole in a condensed and somewhat different form.]

The object of the present essay is to give a summary and connected view of what is known respecting the phenomena and intimate nature of sanguification. For a considerable proportion of the facts, I am, of course, indebted to others; but I flatter myself that my readers will readily excuse the introduction of these, on reflecting that the assistance of what is known is necessary to the further extension of knowledge, and to enable us to arrive at the unknown.

Perhaps, it may facilitate the perusal of these pages to premise, in general terms, the opinion which my observations have led me to form respecting the development and nature of the blood, the arrangement of the subject being chiefly founded upon that opinion. My notion is then that the blood begins to be formed, or developed from the food, in all its parts from the first moment of its entrance into the duodenum, or even, perhaps, from the first moment of digestion, and that it gradually becomes more and more perfect as it passes through the different stages to which it is subjected, till its formation be completed in the sanguiferous tubes, when it represents an aqueous solution of the principal textures and other parts of the animal body to which it belongs.

The chief ingredients in the blood are *albumen*, *fibrin*, and the *colouring principle*, which may be supposed to represent the *common cellular texture*, the *muscular texture*, and the *nervous texture*,* respectively. These different principles are so nearly allied to one another in their chemical properties, that Berzelius has given them the general name of albuminous contents of the blood—a term which, for the sake of convenience, we shall adopt in the following inquiry.

The principal distinct stages in the formation of blood in all the more perfect animals are *digestion*, *chymification*, *chylification*, and *sanguification*, usually so called; the first process being confined to the stomach, the second to the duodenum, the third to the lacteals, and the fourth to the blood vessels.

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* I by no means wish to be understood to assert that the red particles of the blood are destined to form the cerebral and nervous substances of animal bodies. I believe, however, that they are more intimately connected with the nervous function than any other ingredient of the blood, as I shall attempt to show hereafter.

The properties of *chyme*,* *chyle*, and *blood*, the results of these processes, appear to run gradually and imperceptibly into one another, and hence, perhaps, they can hardly be considered as distinct and well defined steps in the general process of sanguification. As, however, the vessels, or organs, in which they take place are perfectly distinct, it becomes a matter of convenience to consider the processes themselves as distinct also. I shall, therefore, first consider the important function of *digestion*.

Phenomena, &c. of Digestion in a Rabbit.—A rabbit which had been kept without food for 12 hours was fed upon a mixture of bran and oats. About two hours afterwards, it was killed, and examined immediately while still warm; when the following circumstances were noticed:—The stomach was moderately distended, with a pulpy mass, which consisted of the food in a minute state of division, and so intimately mixed, that the different articles of which it was composed could be barely recognized. The digestive process, however, did not appear to have taken place equally throughout the mass, but seemed to be confined principally to the superficies, or where it was in contact with the stomach. The smell of this mass was peculiar, and difficult to be described. It might be denominated fatuous and disagreeable. On being wrapped up in a piece of linen and subjected to moderate pressure, it yielded upwards of half a fluid ounce of an opaque, reddish-brown fluid, which instantly reddened litmus paper very strongly, though not permanently, as upon being dried, or even exposed to the air for a short time, the blue colour was restored.† It instantly coagulated milk, and, moreover, seemed to possess the property of redissolving the curd, and converting it into a fluid, very similar to itself in appearance. It was not coagulated by heat, or acids; and, in short, did not exhibit *any evidence of an albuminous principle.* On being evaporated to dryness, and burned, it yielded very copious traces of an alkaline muriate, with slight traces of an alkaline phosphate and sulphate; also of various earthy salts, as the sulphate, phosphate, and carbonate of lime.

Very similar phenomena were observed in other instances. The contents of the stomach uniformly reddened litmus paper, and, in general, coagulated milk (except in one instance, in which the animal had lately died, apparently from some injury of the stomach, which was quite crammed with food), when the property of acting upon milk was very weak, and appeared to be either neutralized or destroyed. In this instance also, the inner coat of the stomach, especially in the neighbourhood of the pylorus, was dissolved.*

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* I use the term *chyme* in a sense somewhat different from that commonly employed, by limiting it to that portion of the alimentary matter found in the duodenum, which has already, or is about to become albumen, and thus to constitute a part of the future blood.

† On looking at the litmus paper the next day, I observed it had again assumed a deep red colour, which was permanent. This curious fact will be noticed hereafter.

Phenomena of Digestion in a Pigeon.—The animal, which was the subject of the present examination, was young, but fully fledged, and had been fed about two hours before it was killed upon a mixture of barley and peas. It was opened and examined immediately after death. In the *crop* was a portion of the food, which was swollen and soft, but appeared to have undergone no further sensible change than what might have been expected from mere heat and moisture. This organ did not exhibit any indications of the presence of an acid. The *gizzard*, or stomach, contained corn in various states of decomposition, the internal parts of some of the seeds being reduced to a milky pulp, which flowed out on their being subjected to pressure;

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* Since the above observations were published, Dr. Wilson Philip has given a more extended account of the phenomena of the digestive process in this animal; an abstract of which I shall lay before my readers.

"The first thing" says Dr. P. "which strikes the eye on inspecting the stomachs of rabbits which have lately eaten is, that the new is never mixed with the old food. The former is always found in the centre surrounded on all sides by the old food, except that on the upper part between the new food and the smaller curvature of the stomach, there is sometimes little or no old food. If the old and the new food are of different kinds, and the animal be killed after taking the latter, unless a great length of time has elapsed after taking it, the line of separation is perfectly evident, so that the old may be removed without disturbing the new food.

"If the old and the new food be of the same kind, and the animal is allowed to live for a considerable time after taking the latter, the gastric juice passing from the old to the new food, and changing as it pervades it, renders the line of separation indistinct; but towards the small curvature of the stomach, and still more towards the centre of the new food, we find it, unless it has been very long in the stomach, comparatively fresh and undisturbed. All around, the nearer the food lies to the surface of the stomach the more it is digested. This is true even with regard to the small curvature compared with the food near the centre, and the food which touches the surface of the stomach is always more digested than any other found in the same part of the stomach. But unless the animal has not eaten for a great length of time, it is in very different stages in different parts of the stomach. It is least digested in the small curvature, more in the large one, and still more in the middle of the great curvature.

"These observations apply to the cardiac portion of the stomach." "The food in the pyloric portion of the stomach of the rabbit is always found in a state very different from that just described. It is more equally digested, the central parts differing less in this respect from those which lie next the surface of the stomach." "One of the most remarkable differences between the state of the food in the cardiac and pyloric portions of the stomach is, that in the latter it is comparatively dry; in the former, mixed with a large proportion of fluid, particularly when digestion is pretty far advanced, and time consequently has been given for a considerable secretion from the stomach."

Thus continues Dr. Philip: "It appears that in proportion as the food is digested, it is moved along the great curvature, when the change in it is rendered more perfect to the pyloric portion. The layer of food lying next the surface of the stomach is first digested. In proportion as this undergoes the proper change, it is moved on by the muscular action of the stomach, and that next in turn succeeds to undergo the same change. Thus a continual motion is going on; that part of the food which lies next the surface of the surface passing towards the pylorus, and the more central parts approaching the surface."

Dr. Philip has remarked, that the great end of the stomach is the part most usually found acted upon by the digestive fluids after death.

others were reduced to a mere husk, while others again were in various states between these two extremes. The whole contents of the stomach exhibited decidedly acid properties; but the litmus paper recovered its blue colour again almost instantly on exposure to the air. They coagulated milk completely, but *yielded no trace of an albuminous principle.*

Phenomena of Digestion in the Tench and Mackerel.—The contents of the stomach and upper intestines of the tench were examined immediately after death. As, however, the animal had been previously kept for a considerable time in an unnatural state, the phenomena observed were not so satisfactory as could have been wished. The contents of the stomach and upper portion of the intestines consisted of little more than a yellowish glairy fluid, which seemed to be bile; and the small portion of alimentary matter present appeared to be unnatural, and little capable of being acted upon by the digestive powers. No traces of an albuminous principle were, therefore, discoverable, nor indeed could be expected to exist in the stomach, or the upper portions of the alimentary canal. The *mackerel,* whose digestive organs were the subject of examination, had just arrived from the coast where it had been caught the day before. The stomach was nearly filled with a whitish grumous mass, in which the undigested bony remains of some small fish were distinctly visible. This mass very faintly reddened litmus; and, by the assistance of heat, coagulated milk. It underwent a sort of partial coagulation by the acetic and other acids, especially when heat was applied; but *no traces of albuminous* matter could be perceived in it.

Phenomena of Chymification.—The examinations of chyme have not been numerous. Dr. Marcet has published a brief account of the chyme of the turkey. I have myself examined the chymes of several different animals: some of the most important of these examinations I shall detail at length; the results of others will be only mentioned. In these examinations my chief object has been to ascertain if the chyme exhibited any traces of the albuminous contents of the blood.

*Comparative Examination of the Contents of the Duodena of two Dogs, one of which had been fed on vegetable Food, the other on animal Food only.**—The chymous mass from vegetable food (principally bread) was composed of a semifluid, opaque, yellowish white part, containing another portion of a similar colour, but firmer consistence, mixed with it. Its specific gravity was 1·056. It showed no traces of a free acid, or alkali; but coagulated milk completely, when assisted by a gentle heat.

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* For the opportunity of making these examinations, as well as those of the chyle afterwards related, I am indebted to the kindness of Mr. Astley Cooper, who, wishing to ascertain the properties of these substances, when preparing his lectures for the Royal College of Surgeons, upwards of four years ago, obligingly furnished me with the materials for making the requisite experiments.

That from animal food was more thick and viscid than that from vegetable food, and its colour was more inclining to red. Its sp. gr. was 1·022. It showed no traces of a free acid, or alkali, nor did it coagulate milk even when assisted by the most favourable circumstances.

On being subjected to analysis, these two specimens were found to consist of

	Chyme from vegetable food.	Chyme from animal food.
Water	86·5	80·0
Gastric principle, united with the alimentary matters, and apparently constituting the chyme, mixed with excrementitious matter	6·0	15·8
Albuminous matter, partly consisting of fibrin, derived from the flesh on which the animal had been fed	—	1·3
Biliary principle	1·6	1·7
Vegetable gluten?	5·0	—
Saline matters	0·7	0·7
Insoluble residuum	0·2	0·5
	100·0	100·0

These results were obtained as follows:

Water.—The quantity of water present was ascertained by evaporating to dryness a known weight of each of the specimens upon a water-bath.

Chymous Principle, &c.—The proportion of this was obtained by adding acetic acid to a known quantity of the mass, and boiling them together for some time. The solid result thus obtained was then collected and dried as before. It consisted partly of a precipitate composed of the digested alimentary matter apparently combined with the gastric principle,* and partly of undissolved and excrementitious alimentary matter. I considered it, therefore, as the chyme in which the albuminous principle was not yet so completely formed, or developed, as to be recognized, mixed with excrementitious matter.

Albuminous Matter, &c.—After the above had been removed by filtration, prussiate of potash was added to the acetic solution, which, in the chyme from vegetable food, produced no precipitate, indicating the *absence* of *albumen;* but in the chyme from animal food, a copious one. The albuminous matter present in the latter, appears to have been partly derived from the flesh on which the animal had been fed.

Biliary Principle.—Both chymes were found to contain this principle. It was separated by digesting alcohol on the dried residuum of the chyme. This took up the biliary principle, which was then obtained by driving off the alcohol. It possessed all the usual properties of this principle, except that it appeared to be less easily miscible with water than in its natural state, and to approach more nearly to the nature of a resin, or adipocire, changes probably induced in it, partly at least, by the action of the alcohol.

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* The nature of the gastric fluids, and particularly of the gastric juice, or principle, will be more fully considered in a subsequent part of this paper.

Vegetable Gluten?—The chyme from vegetable food, which consisted of bread, yielded a portion of a principle soluble in acetic acid, and not precipitable by prussiate of potash nor ammonia. Hence it was not albumen. It was precipitated by solution of potash, and possessed some other properties analogous to vegetable gluten.

Saline Matters.—The salts were obtained by incineration, and consisted chiefly of the muriates, sulphates, and phosphates, as is usual in animal matters.

Insoluble Residuum.—This consisted chiefly in the vegetable chyme of hairs, &c. in the animal chyme, partly of tendinous fibres.

Such is a brief account of these two varieties of what is usually denominated chyme, and as connected with this subject, I shall add here, by way of contrast, a tabular view of the properties of the alimentary matters taken from different portions of two other dogs which had been similarly fed. These various specimens of alimentary matters were treated with the same general views, and consequently nearly in the same manner, as the two varieties of chyme above described, and the results were as follow:

VEGETABLE FOOD.		ANIMAL FOOD.	
1. *Chymous Mass from Duodenum.*		1. *Chymous Mass from Duodenum.*	
Composed of a semifluid, opaque, yellowish white part, having mixed with it another portion of a similar colour, but of firmer consistence. Coagulated milk completely. It consisted of		More thick and viscid than that from vegetable food, and its colour more inclining to red. Did not coagulate milk. Composed of	
A. Water.............	86·5	A. Water.............	80·0
B. Chyme, &c........	6·0	B. Chyme, &c.........	15·8
C. Albuminous matter..	—	C. Albuminous matter ..	1·3
D. Biliary principle.....	1·6	D. Biliary principle.....	1·7
E. Vegetable gluten? ..	5·0	E. Vegetable gluten?...	—
F. Saline matters......	0·7	F. Saline matters	0·7
G. Insoluble residuum ..	0·2	G. Insoluble residuum ..	0·5
	100·0		100·0

VEGETABLE FOOD.	ANIMAL FOOD.
2. *From the Cæcum.*	2. *From the Cæcum.*
Of a yellowish brown colour, and of a thick and somewhat slimy consistence. Did not coagulate milk.	Of a brown colour, and very slimy consistence. Smell very offensive and peculiar. Coagulated milk.
A. Water, quantity not ascertained.	A. Water, quantity not ascertained.
B. Combination of mucous principle, with altered alimentary matters insoluble in acetic acid, and constituting the chief bulk of the substance.	B. Combination of mucous principle, with altered alimentary matters insoluble in acetic acid, and constituting the chief bulk of the substance.
C. Albuminous matter, none.	C. Albuminous matter, a distinct trace.
D. Biliary principle, somewhat altered in quantity, nearly as above.	D. Biliary principle, somewhat altered in quantity, nearly as above.
E. Vegetable gluten? none; but contained a principle soluble in acetic acid, and precipitable very copiously by oxalate of ammonia.	E. Vegetable gluten? none; but contained a principle soluble in acetic acid, and precipitable very copiously by oxalate of ammonia.
F. Saline matters, nearly as above.	F. Saline matters, nearly as above.
G. Insoluble residuum, in small quantity.	G. Insoluble residuum, in small quantity.
3. *From the Colon.*	3. *From the Colon.*
Of a brownish yellow colour, of the consistence of thin mustard, and full of air bubbles. Smell faintish and peculiar, somewhat like raw dough. Did not coagulate milk.	Consisted of a brownish, tremulous, and mucus-like fluid part, with some whitish flakes, somewhat like coagulated albumen, suspended in it. Smell faintish, and not peculiarly fœtid, like bile. Coagulated milk.
A. Water, quantity not ascertained.	A. Water, quantity not ascertained.
B. Combination of mucous principle with altered alimentary matters, the latter in excess, insoluble in acetic acid, and constituting the chief bulk of the substance.	B. Combination of alimentary matter in excess with mucous principle, insoluble in acetic acid, and constituting the chief bulk of the substance.
C. Albuminous matter, none.	C. Albuminous matter, none.
D. Biliary principle, nearly as before in all respects.	D. Biliary principle, nearly as before in all respects.

VEGETABLE FOOD.	ANIMAL FOOD.
E. Vegetable gluten? none, but contained a principle soluble in acetic acid, and copiously precipitable by oxalate of ammonia as in the cœcum.	E. Same as in the cœcum above-mentioned.
F. Salts, nearly as above.	F. Salts, nearly as above. Only some traces of an alkaline phosphate were observed.
G. Insoluble residuum, less than in the cœcum.	G. Insoluble residuum, a flaky matter in very minute quantity.
4. *From the Rectum.*	4. *From the Rectum.*
Of a firm consistence, and of an olive-brown colour inclining to yellow. Smell fœtid and offensive. Did not coagulate milk.	Consisted of firm scybala, of a dark brown colour inclining to chocolate. Smell very fœtid. Milk was coagulated by the water in which it had been diffused.
A. Water, quantity not ascertained.	A. Water, quantity not ascertained.
B. Combination, or mixture of altered alimentary matters in much greater excess than in the colon, with some mucus; insoluble in acetic acid, and constituting the chief bulk of the fæces.	B. Combination, or mixture of altered alimentary matters in much greater excess than in either of the other specimens, with some mucus; insoluble in acetic acid, and constituting the chief bulk of the fæces.
C. Albuminous matter, none.	C. Albuminous matter, none.
D. Biliary principle, partly changed to a perfect resin.	D. Biliary principle, more considerable than in the vegetable fæces, and almost entirely changed to a perfectly resinous-like substance.
E. Vegetable gluten? none; but contained a principle similar to that in the cœcum and colon.	E. Vegetable gluten? none; but contained a principle similar to that in the cœcum and colon.
F. Salts, nearly as before.	F. Salts, nearly as before.
G. Insoluble residuum, consisting chiefly of vegetable fibres mixed with hairs.	G. Insoluble residuum, consisting chiefly of hairs.

Examination of the Contents of the Duodenum of the Ox.—This had been kept for some time before examination, and appeared to contain an unusually large proportion of bile. Its colour was greenish, and it was of a ropy consistence, apparently holding suspended in it some solid matters, which, after a little time, subsided to the bottom. Its taste was bitter, its smell faintish, and somewhat similar to bile. Sp. gr. 1·023. It exhibited very faint traces of an acid, and coagulated milk completely, when assisted by a gentle heat. Nearly the same method was adopted in the analysis of this, as of the other specimens before mentioned, and the results were as follow:

A. Water	91·1
B. Gastric principle, united with alimentary matters, and apparently constituting the chyme, mixed with excrementitious matter	2·0
C. Albuminous matter	—
D. Biliary principle	4·4
E. Picromel?	1·4
F. Vegetable gluten, or extract	—
G. Saline matters	0·8
H. Insoluble residuum	0·3
	100·0

The chymous matter was less in quantity, and the biliary principle much greater in this specimen than in any of the others. There was also a substance present (E) which I have called picromel. It was of a brown colour, and gummy consistence. Taste, first bitter, and afterwards sweetish. Soluble in water, but perfectly insoluble in alcohol. It was obtained after the action of the alcohol by boiling the residuum in distilled water. It was not precipitated by the oxymuriate of mercury, but completely so by the subacetate of lead. Hence it appeared to be a sort of altered mucus, or rather, perhaps, a combination of mucus with a little biliary principle, which the alcohol was incapable of removing. Indeed so intimately does the biliary principle unite with all animal substances with which it comes in contact, that it can scarcely ever be again entirely separated. The insoluble residuum (H) was chiefly vegetable fibres.

Examination of the Contents of the Duodena of Rabbits.—The animals were the same as those in which the phenomena of digestion above described were observed; and the experiments for ascertaining the properties of the contents of their duodena were similar to those made upon the duodenal contents of the dogs and ox; and need not, therefore, be repeated. The duodenum of the rabbit, fed on a mixture of bran and oats above mentioned, at its commencement, contained chiefly a greenish yellow glairy fluid, full of air bubbles, with a small portion only of the insoluble parts of the food. This yielded decided evidence of the existence of a true chymous or albuminous principle. A little lower down in the duodenum a similar glairy fluid was observed, but it was more free from air bubbles, and seemed to contain a larger proportion of an albuminous principle. In short, the quantity of albuminous matter was found to increase to the distance of about six inches from the pylorus, after which it diminished; and at the distance of 24 inches from the pylorus, it was barely perceptible. The contents of the ileum were of a greenish colour, and consisted of a greater proportion of the excrementitious part of the food than the contents of the duodenum. No traces of albuminous matter were found in this portion of the intestinal canal. The cœcum in this animal is very large, and in the present instance was nearly full of a dark brown pultaceous mass, of a feculent odour, and which yielded no traces of albumen. The colon and rectum contained dry brown and hard scybala, apparently consisting of little more than the insoluble parts of the food and some biliary matter. None of the contents of the intestinal canal, from the pylorus downwards, were sensibly acid, or alkaline, nor did they appear capable of coagulating milk.

Very similar phenomena were observed in other instances. But, when the animal was opened at a longer period after feeding, I generally found much stronger evidences of albuminous matter, not only in the duodenum, but nearly throughout the whole of the small intestines. The quantity, however, was generally very minute in the ileum; and where it enters the cœcum, no traces of this principle could be perceived. The general appearances also of the contents of the upper parts of the small intestines were always very similar to those above described; that is to say, they were of a yellowish colour, and of a ropy or glairy consistence, and mixed with some insoluble and excrementitious matter. In the ileum in general the colour was more green, the consistence firmer, and the proportion of excrementitious matters greater. In the cœcum there was always a great collection of feculent matter, which was uniformly similar in all its properties to that before described. The contents of the colon and rectum also were precisely similar in their appearances and properties to those above-mentioned.

Examination of the duodenal Contents of the Pigeon and Turkey.—The pigeon was the same as that employed in observing the phenomena of digestion. Just at the commencement of the duodenum there were numerous air bubbles which exhibited the appearance of having been elicited by effervescence from the contents of the stomach upon their first entry into the intestine. The colour of the contents of this part of the intestine was greenish yellow, and their consistence was thin and glairy, with a mixture, as in the instance of the other animals above-mentioned, of some excrementitious matter. Near the pylorus, faint traces only of albumen were observed; but the quantity increased to about the distance of six inches, and afterwards rapidly diminished; and at 12 inches from the pylorus, no traces of this principle were perceptible; and here the alimentary matters assumed a browner colour and firmer consistence, and appeared to be altogether excrementitious.

The contents of the duodenum of the *turkey* have been examined by Dr. Marcet. He describes them as yielding abundant traces of albumen, and states that on being burned, they left a saline residuum of about six parts in a thousand of the original mass, "amongst which the presence of iron, lime, and an alkaline muriate was clearly ascertained."

Examination of the duodenal Contents of the Tench and Mackerel.—From my being unable to procure fish in their natural state, my examinations of these animals have not been so satisfactory as could be desired. In the upper portion of the intestinal canal of the tench, which had been kept, as before observed, for some time in an unnatural state, no traces of an albuminous principle could be perceived; but lower down, where the alimentary matter was more abundant, I thought some traces of this principle were perceptible. In this animal, none of the substances found in the canal were sensibly acid, or alkaline, nor coagulated milk. In the mackerel, the contents of the duodenum and upper intestines very closely resembled those of the stomach, both in their appearance and properties, except that they were of a more glairy consistence, especially about the pyloric cœca, and gave some faint indications of what I considered as an albuminous principle.

Properties of the Chyle.—I now proceed to describe, as far as they are known, the properties of the chyle in three different stages of its progress towards the sanguiferous system; namely, as it exists in the absorbent vessels, or lacteals, near the intestines, as it exists in the same vessels near the thoracic duct, and as it exists in the thoracic duct itself.

Owing to the minuteness of the lacteal vessels, and the consequent difficulty of collecting their contents in any quantity, the properties of the chyle, as it exists immediately after it has been absorbed from the intestines, are but imperfectly known. In the mammalia, it is opaque, and white like milk. In birds and fishes, on the contrary, it is nearly transparent and colourless. The only examinations of chyle in this state of its formation, that I am acquainted with, are those of Emmert and Reuss,* which were made upon the chyle of the horse. It differed from perfect chyle taken from the thoracic duct, in being more white and opaque, in undergoing spontaneous coagulation much more slowly and imperfectly, and in not assuming a reddish colour on exposure to the air: hence it appeared to contain a very small proportion only of a principle analogous to fibrin, or, at least, this principle existed as yet in a very imperfect state, and no colouring principle.

Chyle from the sublumbar branches of horses has been examined by Emmert and Reuss, and likewise by Vauquelin.†

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* See Annales de Chimie, tom. lxxx. p. 81.

† See Annales de Chimie, tom. lxxxi. p. 113; also *Annals of Philosophy*, ii. 220.

These chemists agree in representing its properties as more imperfect and ill defined than those of chyle taken from the thoracic duct. That examined by Emmert and Reuss, when compared with chyle taken from the thoracic duct, was found to undergo spontaneous coagulation much more imperfectly. Its colour was white, with minute yellow globules swimming in it. But in a few hours there was observed in it a little reddish mass swimming in a yellowish fluid, which, after some days, disappeared, and assumed the form of a sediment at the bottom of the vessel. The specimen examined by Vauquelin was white and opaque like milk, and it contained a coagulum equally white and opaque. This coagulum was considered as imperfectly formed fibrin; and in the chyle examined by Emmert and Reuss, constituted about one per cent. of the whole fluid. Both specimens also were found to contain albumen, the usual salts of the blood, and also a peculiar principle, the properties of which will be considered hereafter.

Chyle from the thoracic duct has been often examined, and with very similar results. If an animal be killed a few hours after having taken food, and immediately opened, and the thoracic duct pierced, the chyle being now in a perfectly fluid state will flow out readily. Its colour at this time is nearly white. Its taste faintly saline and sweetish. Its smell peculiar; it has been compared by Emmert and Reuss to that of the *σπέρμα virile*. In a period of time somewhat different in different instances, but generally in a few minutes, it begins to assume a gelatinous appearance, and to undergo coagulation; the colour also, if it has been exposed to the air, changes to a faint red, or pink. The time requisite to produce the maximum effect of these spontaneous changes is different; sometimes an hour appears sufficient; generally, however, a much longer time is necessary. In this coagulated state, and often many hours, or even days after it has been removed from the body, it has, in every instance in which I am acquainted, been examined by chemists; and the following observations, therefore, are to be understood to apply to it in this condition only.

It would be loss of time to mention the opinions of the older physiologists in chyle. All the modern chemists have considered it as very analogous to the blood. The experiments of Emmert and Reuss and Vauquelin, above-mentioned, establish this point in the most satisfactory manner; and those of others to the same effect might be mentioned if necessary. I shall only, therefore, detail a very few experiments. The most recent examinations of chyle are those of Dr. Marcet * and myself, of the chyles of two dogs, one of which had been fed entirely on vegetable food, the other on animal food. The experiments were made now upwards four years ago, at the request of Mr. Astley Cooper; and the chyles which I examined were, I believe, taken from the same two dogs, the contents of whose duodena have been described in a former part of this paper.

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* See Med. Chirurg. Transactions, vi. 618.

Chyle of a Dog fed on Vegetable Food.—This is described by Dr. Marcet as appearing "a short time after being collected in the form of a semitransparent, inodorous, colourless fluid, having but a very slight milky hue, like whey diluted with water. Within this fluid there was a coagulum, or globular mass, which was also semitransparent, and nearly colourless, having the appearance and consistence of *albumen ovi*, or of those gelatinized transparent clots of albuminous matter which are sometimes secreted by inflamed surfaces. This mass had a faint pink hue, and minute reddish filaments were observed on its surface." To this description I have nothing to add, except that the specimen I examined did not sensibly affect litmus, or turmeric papers, in any state, nor coagulate milk. Dr. Marcet's further observations also agree with my own. He found that the coagulum, when separated from the serum, parted readily with its serosity, or fluid portion, and was at length reduced to a very small size. The sp. gr. of the serum he found to be in different instances 1·0215 and 1·022. He appears to have considered the serum as well as the coagulum to have contained albumen. The portion of solid matter, including salts, varied in different specimens of the chyle from 4·8 to 7·8 per cent. The proportion of saline matter was very uniformly about 0·92 per cent.

Chyle of a Dog fed on Animal Food.—Dr. Marcet's description of this species of chyle agrees also with my own observations. He describes it as resembling the last, except that "instead of being nearly transparent and colourless, it was white and opaque like cream. The coagulum was also white and opaque, and had a more distinct pink hue, with an appearance not unlike that of very minute blood-vessels. The coagulum, as in the former instance, gradually yielded further quantities of serous fluid till nothing remained but a small quantity of a pulpy opaque substance, in appearance somewhat similar to thick cream, and containing minute globules, besides the red particles above noticed. The residue of the coagulum became in the course of three days quite putrid, whilst that obtained from vegetable chyle in a similar manner had not yet begun to undergo that process." The serous portion on standing assumed a creamy-like appearance on its surface. Its sp. gr. and other properties were similar to those from vegetable food. It left a quantity of solid matter, including salts, varying in different specimens from 7·0 to 9·5 per cent. The proportion of saline matter were the same as before.

The following are the results of my examinations of these two varieties of chyle:

	Vegetable food.	Animal food
Water	93·6	89·2
Fibrin	0·6	0·8
Incipient albumen ?	4·6	4·7
Albumen, with a little red colouring matter	0·4	4·6
Sugar of milk ?	trace	—
Oily matter	trace	trace
Saline matters	0·8	0·7
	100·0	100·0

Nearly the same modes of operating were adopted in the examinations of these specimens of chyle as in those of the chymes, formerly described: thus,

The quantity of *water* was ascertained, as in the former instances, by evaporating a known weight of the perfect chyle to dryness, on a water-bath. The coagulum of the chyle was repeatedly washed with cold water till it ceased to give off any thing to that fluid; the remainder was a small portion of a substance differing in very slight particulars only from the fibrin of the blood. One of the chief of these differences was its greater difficulty of solubility in dilute acetic acid. It was, therefore, considered as *fibrin*.

To the serous portion was added dilute acetic acid, and heat applied, till the mixture boiled. A copious precipitation took place, which, therefore, was not albumen. It differed also from the caseous principle of milk, since it was readily and completely precipitated by the oxymuriate of mercury. It was named *incipient* albumen, and its nature will be more fully considered hereafter.

After the above principle had been removed by filtration, prussiate of potash was added to the acetic solution. A copious precipitate fell, which was considered as *albumen*.

In the serum of the vegetable chyle there appeared a trace of what was considered as sugar of milk. This was not observed in the serum of the animal chyle.

In both chyles, but especially in that from animal food, there was a distinct trace of an oily substance.

The saline matters consisted chiefly of the alkaline muriates, with traces of a sulphate, and, perhaps, of a lactate; but of this last I am not certain.

The chyles of birds, fishes, and the inferior animals, have not, as far as I know, been examined. Their properties, therefore, at present, are entirely unknown, which is much to be regretted.

(*To be continued.*)

Article V.

Experiments on Muriatic Acid Gas, with Observations on its Chemical Constitution, and on some other Subjects of Chemical Theory. By John Murray, M.D. F.R.S.E. Fellow of the Royal College of Physicians of Edinburgh.*

Some years ago I proposed, as decisive of the question which has been the subject of controversy on the nature of oxymuriatic and muriatic acids, the experiment of procuring water from muriate of ammonia, formed by the combination of dry ammoniacal and muriatic acid gases. Muriatic acid gas being the sole product of the mutual action of oxymuriatic gas and hydrogen, it follows, that if oxymuriatic gas contain oxygen, muriatic acid gas must contain combined water; while, if the former be a simple body, the latter must be the real acid, free from water. When muriatic acid gas is submitted to the action of substances which combine with acids, water is obtained; but though the most simple and direct conclusion from this is, that the water is deposited from the muriatic acid gas, the result may be accounted for on the opposite doctrine, by the supposition that it is water formed by the combination of the hydrogen of the acid with the oxygen of the base. Ammonia, however, containing no oxygen, if water is obtained from its combination with muriatic acid gas, we obtain a result which cannot be accounted for on this hypothesis, but must be regarded as a proof of the presence of water in the acid gas. And this again affords a proof equally conclusive of the existence of oxygen in oxymuriatic gas.

The results of the experiment which I had brought forward were involved in much controversial discussion: and a brief recapitulation of the objections that were urged to it is necessary, as an introduction to the experiments I have now to submit; and to the consideration of the present state of the question.

The original experiment was performed by combining thirty cubic inches of muriatic acid gas with the same volume of ammoniacal gas carefully dried. The salt formed was exposed in a small retort with a receiver adapted to it to a moderate heat gradually raised. Moisture speedily condensed in the neck of the retort, which increased and collected into small globules.†

This result was admitted by those who defended the new doctrine, when the experiment was performed in the manner I have described—water being obtained, it was allowed "in no inconsiderable quantity." But, to obviate the conclusion, it was asserted, that this is water which has been absorbed by the salt from the atmosphere. This was affirmed by Sir Humphry Davy, who stated that the salt absorbs water in this manner to a very considerable extent; that it is only from the salt in this state that water can be procured, and that when it is formed from the combination of the gases in a close vessel, and heated without exposure to the air, not the slightest trace of water appears, even when the experiment is performed on a large scale.

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* From the Transactions of the Royal Society of Edinburgh, vol. viii. part ii.

† Nicholson's Journal, xxxi. 126.

The reverse of this I was able to demonstrate by further experimental investigations. It was shown that the salt absorbs no moisture from the air in the common state of dryness and temperature in which the experiment is performed: when weighed immediately on its formation, in an exhausted vessel, it gains no weight from exposure, but remains the same after a number of hours; and when exposed to the air in the freest manner, it remains, after many days, perfectly dry. It was further shown, that when the other circumstances of the experiment are the same, it yields no larger portion of water when it has been exposed to the air than it does without this previous exposure. And, lastly, it was proved, that when the salt has been formed, and is heated without the air having been admitted, water is obtained from it. This last result was even at length admitted by those who had advanced the opposite assertion, in an experiment performed with a view to determine the fact. The quantity of water was indeed less than what is procured in the other mode; but this was obviously owing to the circumstances of the experiment being unfavourable to its expulsion, more particularly to the difficulty of applying a regulated temperature to a thin crust of salt, so as to separate the water without volatilizing the salt itself, and to the effect arising from the whole internal surface of a large vessel being encrusted with the salt, so that if the heat is locally applied, the aqueous vapour expelled from one part is in a great measure condensed and absorbed at another; or if the heat is applied equally, is retained in the elastic form, and, as it is cooled, is equally condensed. Accordingly, when the experiment was repeated, obviating these sources of error as far as possible, the water obtained was in larger quantity. And as no fallacy belongs to the conducting the experiment in the more favourable mode in which it was first performed (the assertion of the absorption of water from the air being altogether unfounded), the quantity procured in that mode is to be regarded as the real result.*

The argument was maintained that the water might be derived from hygrometric vapour in the gases submitted to experiment. This it was easy to refute. Dr. Henry had shown that ammonia, after exposure to potash, and muriatic acid after exposure to muriate of lime, retain no trace of vapour whatever; and these precautions had been very carefully observed. The assertion was brought forward too only to account for the minute quantity of water obtained in that mode of conducting the experiment which affords the least favourable result, and were it even admitted to all the extent to which it can be supposed to exist, is inadequate to account for the larger quantity obtained in the other.

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* Nicholson's Journal, xxxii. 186, &c.; xxxiv. 271.

That the entire quantity of water contained in the muriatic acid gas is not to be looked for is evident from the nature of the ammoniacal salt, particularly its volatility, whence the due degree of heat to effect the separation of the water cannot be applied. If the other muriates yield the greater part of their water, only when raised nearly to a red heat (which is the case), it is not to be supposed that muriate of ammonia shall do so at a temperature so much lower as that which it can sustain without volatilization. What is to be expected is a certain portion of water, greater as the arrangements employed are better adapted to obviate the peculiar difficulty attending the experiment. There *is* a production of water in every form of it; and there exists no just argument whence it can be inferred that the quantity is less than what ought to be obtained. On the opposite doctrine, none whatever should appear.

To effect the more perfect separation of the water from the muriate of ammonia, I had performed the additional experiment of passing the salt formed from the combination of the two gases, in vapour through ignited charcoal, on the principle that, by the interposition of the charcoal, the transmission of the vapour would be impeded, and it would be exposed to a more extensive surface, at which a high temperature would operate, while some effect might also be obtained from the affinities exerted by the carbonaceous matter. To remove any ambiguity from the effect of the charcoal, it was previously exposed in an iron tube, to a very intense heat, until all production of elastic fluid had ceased; and removed while still warm into a tube of Wedgewood's porcelain, containing the muriate of ammonia, which was then placed across a furnace so as to be raised to a red heat. As soon as the vapour of the salt passed through the ignited charcoal, gas was disengaged, which was conveyed by a curved glass tube adapted to the porcelain one, and received in a jar over quicksilver. Moisture was at the same time pretty copiously deposited, condensing both in the glass tube in globules, and being brought in vapour with the gas, which it rendered opaque, and condensing on the surface of the quicksilver within the jars. The elastic fluid consisted of carburetted hydrogen and carbonic acid, products evidently of the decomposition by the ignited charcoal of a portion of the liberated water. In this experiment, then, the result was still more satisfactory than in the other. That no ambiguity arose from any effect of the charcoal in affording water, is evident from this, that the water appeared at the moment the salt began to pass in vapour, and at a temperature far below that at which the charcoal had ceased to afford any gas. In another variation of the experiment, muriate of ammonia was passed in vapour through an ignited porcelain tube alone. Water was obtained in larger quantity than when the salt had been exposed to a heat short of its volatilization; and even the salt which had yielded water by that operation afforded an additional quantity in this mode—a proof of the more perfect separation of the water by the effect of a higher temperature.*

By all these results, then, I consider the existence of water in muriate of ammonia, and, of course, in muriatic acid gas, as demonstrated.

Dr. Ure has lately laid before the Society the result of another mode of conducting the experiment—that of subliming the muriate of ammonia over some of the metals, at the temperature of ignition. Water is thus stated to be obtained in considerable quantity, with a production of hydrogen gas.

No objection appeared to Dr. Ure's experiment, except, perhaps, that the salt operated on was not that formed by the direct combination of its constituent gases, but the common sal ammoniac, in which water might be supposed to exist, either as an essential, or an adventitious ingredient, as it is abundantly supplied to it in the processes by which it is formed. I had found, indeed, in some of my former experiments,† that sal ammoniac yields no water when exposed to a heat sufficient to sublime it, but affords it only when exposed to a red heat by transmission of its vapour through an ignited tube, that, therefore (owing no doubt to its previous sublimation), it contains apparently even less water than the salt formed by the combination of the two gases. Still objections, entitled to less consideration than this one, had been maintained in the course of this controversy. I, therefore, thought it right to repeat the experiment, with the necessary precaution to obviate it, and to observe the actual result.

Thirty grains of muriate of ammonia, formed from the combination of muriatic acid and ammoniacal gases, were put into a glass tube with a slight curvature. Two hundred grains of clean and dry iron filings were placed over it. The tube was put in a case of iron with sand, and placed across a small furnace, so that the middle part, where the iron filings were, was at a red heat, the extremity terminating in the mercurial trough. The salt, from the heat reaching the closed extremity of the tube, soon passed in vapour through the ignited iron. Gas issued from the extremity, and moisture appeared in the cold part of the tube. A large quantity of gas was collected, which had the odour quite strong of muriatic acid, and was in part condensed by water; the residue burned with the flame of hydrogen. The tube, for several inches, was studded with globules of water, and was bedimmed with vapour further. I did not prosecute the experiment, so as to ascertain the weight of water produced, as I had other experiments in view which I conceived might afford more conclusive results. But it proves the point it was designed to establish, that water is obtained from the salt formed by the combination of the gases, as well as from the common sal ammoniac.

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* Nicholson's Journal, xxxi. 128.

† Id. xxxiv. 274.

My attention having been thus recalled to the subject, I have again executed the experiment in its original and simplest form, that of obtainining water from the salt by heat alone; and to this I was led more particularly, as it had occurred to me that a more perfect abstraction of its water might be effected, by conducting the experiment in an apparatus somewhat on the principle of the instrument invented by Dr. Wollaston, which he named the Cryophorus. In a retort of the capacity of seven cubic inches, fitted with a stop-cock, and exhausted, 60 cubic inches of ammoniacal gas were combined with the requisite quantity of muriatic acid gas, each previously carefully dried, the former by exposure to potash, the latter by exposure to muriate of lime. The stop-cock was then detached from the retort; the excess of ammoniacal gas was removed by a caoutchouc bottle, and replaced by atmospheric air; the salt was pushed down from the neck; and it was connected with another similar retort, the joining of the two being secured by cement. This last retort was also fitted with a stop-cock adapted to a tubulature at its curvature, and heat being applied to it, a little of the included air was allowed to escape. It was then placed in a mixture of muriate of lime and ice; while the other, containing the muriate of ammonia, was placed in warm oil. The heat of this was raised to 420° of Fahr. moisture condensed at the upper part of the neck, when the heat had been raised to 220°, and continued for some time to increase. It then diminished, from the continued application of the heat, carrying it forward into the cold retort; and at the end of the experiment, a considerable part of the body of this was encrusted with a thin film of ice. This result, therefore, coincides entirely with what had been before obtained.*

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* A foreign chemist, who has continued to support the old doctrine of the nature of muriatic acid, has observed (*Annals of Philosophy*, viii. 204) that the water of the muriatic acid gas cannot be supposed to be obtained by the combination of the acid with ammonia; for no neutral ammoniacal salt, he adds, can be obtained free from water, and the water of the acid gas becomes the water essential to the salt. I did not think it necessary to make any reply to this observation, founded entirely, as it appeared to me, on a mistaken assumption. But I may take this opportunity of remarking, that there is no necessary truth in the supposition that the ammoniacal salts must contain water which they cannot yield. When acids combine with bases, the water of the acid does not necessarily remain in the compound. On the contrary, it is capable of being driven off from the greater number of them, by an elevated temperature; and there is no principle on which it can be inferred that ammonia should in this respect be different from other bases. That it is incapable, as the same chemist remarks (*Annals*, vii. 434), of combining with a dry acid, so as to form a neutral compound, is of no weight; for the same thing is true of other bases, which yet, when combined with such an acid by the aid of water, allow this water to escape from the combination. He himself observes, that well-burned

"Another form of experiment occurred to me still more direct and simple, that of transmitting muriatic acid in its gaseous form over ignited metals. If water be obtained in this experiment, it is a result which would prove subversive of the new doctrine; for muriatic acid gas is held to be the real acid, free from water; and the only change which can happen is that of the metal decomposing the acid attracting its chlorine and liberating its hydrogen. And the experiment is further free from the only resource which remained to the advocates of that doctrine, in the case of water being obtained from muriate of ammonia, that it might be derived from the decomposition of the elements of ammonia, regarding it as an alkali containing oxygen. If water were really obtained from the combination of muriatic acid and ammoniacal gases, it would rather indicate, it was said, the decomposition of nitrogen than the existence of water as a constituent of muriatic acid. No weight, I believe, is due to such an assumption; but if any importance were attached to it, it is precluded if water is obtained from the action of metals on muriatic acid gas.

I have executed the experiment in several forms; and in all with a more or less satisfactory result.

One hundred grains of iron filings, clean and dry, were strewed for a length of five or six inches, in a glass tube, which was placed in an iron case across a small furnace, so as to admit of being raised to a red heat. This tube, of about two feet in length, was connected with a wide tube eight inches long, containing dry and warm muriate of lime; and this was further connected at its other extremity, with a retort affording muriatic acid gas, from a mixture of supersulphate of potash and muriate of soda. The open extremity of the long tube, dipped by a slight curvature in quicksilver. On the iron being raised to ignition, and the transmission of the acid gas being conducted slowly, elastic fluid escaped from the extremity of the tube, which was found to be hydrogen; and though no trace of moisture appeared in the anterior part of the tube, it immediately condensed in that part which was cold, beyond the iron filings.

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lime, free from water, does not absorb dry carbonic acid gas, but absorbs it rapidly if aqueous vapour be admitted, though water is not retained in the composition of carbonate of lime. And I have found that dry magnesia does not absorb muriatic acid gas, though, with the aid of water, it forms a combination from which the water can be expelled by heat. That ammoniacal salts exist without water is evident from the combination of carbonic acid gas and ammoniacal gas being effected with the greatest facility; and the circumstance that this compound is not neutral is one not depending on the peculiarity of the ammonia, and its not containing water, like other bases, but on that of the carbonic acid, which, with all the alkalies, even where water is present, has a tendency to form compounds with excess of base. The reason why the ammoniacal salts do not yield the combined water of their acids so completely as that of other salts, is, that from their volatility, or their susceptibility of decomposition, they do not bear that degree of heat which is necessary to produce it. I cannot, therefore, but consider the observation alluded to as one altogether unfounded, and which ought not, on a mere speculation, to have been brought forward against a positive result.

This accumulated in globules, and at length ran into a small portion in the bottom; the sides were bedewed for a length of six inches, and a thin film of moisture appeared beyond nearly its whole length.

By the muriatic acid gas being extricated in the preceding experiment from nearly dry materials, and by its previous transmission over an extensive surface of loose muriate of lime, it was inferred that it would be free from hygrometric vapour; and that it held no moisture, was apparent from no trace of it appearing in the anterior portion of the tube. To obviate, however, entirely, any supposed fallacy from this source, the experiment was performed in the following manner. One hundred grains of clean and perfectly dry iron filings were put into a long glass tube, which was placed, as before, across a small furnace. Muriatic acid gas had been kept in contact with dry muriate of lime for three days, in a jar with a stop-cock adapted to it. This was connected by a short tube with a caoutchouc collar, with the tube containing the iron filings; and a little of the muriatic acid gas being passed through the tube to expel the air, the temperature was raised to ignition. The slow transmission of the gas was continued by the pressure of the mercury in the quicksilver trough, and fresh quantities, which had been equally with the other exposed to muriate of lime, were added, as was necessary. Water almost immediately appeared in the tube beyond the iron filings; it collected in spherules, and continued to accumulate as the gas continued to be transmitted for a length of about seven inches. A portion of the gas, which escaped from the extremity, was clouded, and deposited a film of moisture on the sides of the jar in which it was received over quicksilver. The quantity of gas transmitted amounted to about 35 cubic inches.

There are some difficulties in conducting the experiment in the manner now described, from the consolidation of the metallic matter, and the volatilization of the product. It was also of some importance to vary the experiment. I, therefore, performed it in another mode. Metals scarcely act on muriatic acid gas at natural temperatures, but from such a degree of heat as could be applied by a small lamp, both iron and zinc were acted on; the gas suffered diminution of volume, hydrogen was formed, and a sensible production of moisture took place. The simplest mode of exhibiting this is to introduce iron or zinc filings, previously dry and warm, into a retort fitted with a stop-cock, exhausting it; then admitting dry muriatic acid gas, and applying heat by a small lamp to the filings in the under part of the body of the retort. Moisture soon appears at its curvature in small globules, and increases on successive applications of the heat with the admission of the requisite quantities of gas.

To conduct the experiment, however, on a larger scale, I employed a different apparatus. A tubulated retort, of the capacity of 25 cubic inches, was connected with a jar, containing muriatic acid gas in contact with muriate of lime on the shelf of the mercurial trough, by a tube bent twice at right angles, and fitted by its shorter leg with a collar of caoutchouc to a stop-cock at the top of the jar, its longer leg passing into the tubulature of the retort, so as to terminate within an inch of its bottom, and the joinings being rendered air-tight. The retort is so placed that heat can be applied by a lamp to the bottom, and its neck dips, by a short curved tube, under a jar filled with quicksilver, which, by the reverted position of the retort, may be placed beside the other on the shelf of the trough. At the commencement of the experiment, the metallic filings, previously dry and warm, having been put into the retort, the atmospheric air is expelled by a moderate heat, and small portions of the muriatic acid gas are admitted until the retort is filled with the pure gas. The stop-cock is then closed, and heat is applied by a lamp to the bottom of the retort under a considerable pressure of mercury; any small portion of gas expelled at the extremity being received in the small jar. The heat can thus be successively cautiously applied, and this, as the experiment proceeds, to a greater extent, in consequence of the diminution of volume that takes place. Fresh quantities of muriatic acid gas are admitted from time to time from the jar, and the stop-cock being closed when the heat is applied, the hydrogen gas produced is expelled with any muriatic acid gas not acted on.

In the principal experiment I employed, zinc filings were used in preference to iron, from the consideration that muriate of zinc is less volatile than muriate of iron, and, therefore, would admit of a higher heat being applied to expel any water. One hundred grains of clean and dry zinc filings were introduced while warm into the retort; the air was expelled, and muriatic acid gas was admitted from the jar. On applying heat to the zinc, the retort, which was before perfectly dry, was bedimmed with moisture at its curvature, and small spherules collected at the top of the neck. These increased in size, and extended further as the experiment advanced. After a certain time, part of this disappeared in the interval of cooling, being absorbed by the deliquescent product; but when the heat was again applied, it was renewed, and this in increased quantity, until at length, at the end of four days, during which heat had been frequently applied, the whole tube of the retort, seven inches in length, was studded with small globules of fluid. When the heat had been raised high, a beautiful arborescent crystallization appeared in a thin film on the body of the retort, but no part of this reached the neck. The retort was now detached; the gas it contained was withdrawn by a caoutchouc bottle; a small receiver was adapted; and a slight heat having been applied to expel a little of the air, the joining was made close by cement. The receiver was surrounded with a freezing mixture, and heat was applied by a choffer to the retort, as far as could be done, without raising dense vapours. Globules of liquid, perfectly limpid, collected pretty copiously towards the middle and lower part of the neck, and the receiver, on being removed from the freezing mixture, was covered internally with a film of moisture. The globules in the neck of the retort were absorbed by a slip of bibulous paper, and the quantity was found to amount to 1·2 gr. The receiver being dried carefully and weighed, lost by the dissipation of the moisture within 0·4 gr. Distilled water, in which the bibulous paper was immersed, was quite acid; it gave no sensible turbidness on the addition of ammonia, or of carbonate of soda, and held dissolved, therefore, merely pure muriatic acid. The mass in the retort was of a grey colour, with metallic lustre, in loosely aggregated laminæ somewhat flexible. It weighed 114·8 gr. Adding to this increase of weight which the zinc had gained the weight of the water and the hydrogen gas expelled, it gives a consumption of muriatic acid gas of about 16·8 gr. equivalent to about 43 cubic inches. Supposing the weight of water to be doubled, or nearly so, by saturation with muriatic acid, this gives the product of water in the experiment as equal to nearly one gr.; or about $\frac{1}{3}$th of the whole quantity of combined water which muriatic acid gas is calculated to contain.*

In all the preceding experiments, water has been procured from muriatic acid gas. It is obvious that such a result cannot be accounted for on the hypothesis that it is the real acid free from water, a compound merely of chlorine and hydrogen. On the opposite doctrine, as muriatic acid in its gaseous form is held to contain water, it may be supposed to afford a portion of it.

It may be maintained, however, in this, as it was in the experiment of obtaining water from the muriate of ammonia by heat, that the water produced is derived from hygrometric vapour in the gas. To obviate this, it is sufficient to recur to the fact established by the experiments of Henry and Gay-Lussac, that muriatic acid gas contains no hygrometric vapour; and to the obvious result in the experiment that no quantity that can be assumed would be adequate to account for the quantity actually obtained. The circumstances of the experiment too are such as to preclude any such supposition; and this more peculiarly so than in the experiment of obtaining water from the muriate of ammonia by heat; for in the present case the acid gas is alone employed, while in the other there is an additional equal volume of ammoniacal gas, which may be supposed to afford a double quantity of hygrometric vapour. In the latter, both the gases are condensed into a solid product, and any hygrometric vapour may be supposed to be liberated; but in the present experiment, there remains the hydrogen gas capable of containing hygrometric vapour, while the muriatic acid gas contains none; and the quantity of it thus transmitted over the humid surface, and expelled from the apparatus, must have carried off more vapour than the other, introduced at a lower temperature, could have conveyed. These circumstances, independent of the quantity of water deposited, precluded the supposition of any deposition from the condensation of hygrometric vapour; and there is no other external source whence it can be derived. In this respect, nothing can be more satisfactory than the experiment with the zinc in the apparatus described. The muriatic acid gas rises from dry mercury in contact with muriate of lime, passes through a narrow bent tube, 30 inches in length, without exhibiting the slightest film of moisture, is received into the retort perfectly dry; and when the action of the metal on it is excited by heat, humidity immediately becomes apparent in the curvature of the retort, and this even while the gas is warm, and of course capable of containing more water dissolved than it could do in its former state; and the quantity increases as the experiment proceeds. No arrangement can be supposed better adapted to prove that any deposition of water must be by separation from its existence in the gas in a combined state.

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* The action of the metals on the muriatic acid gas taking place in the above experiments at a heat comparatively moderate, it occurred to me that they might exert a similar action with no higher heat on the acid in muriate of ammonia, and that this might afford an easy mode of exhibiting the results. I accordingly found that on mixing different metals with sal ammoniac in powder, previously exposed to a subliming heat, and exposing the mixture to heat by a lamp, so regulated as to be short of volatilization, the salt was decomposed, ammoniacal gas was expelled, and moisture condensed in the neck of the retort, covering a space of several inches with small globules, and at length running down. The metals I employed were iron, zinc, tin, and lead; 100, 150, or 200 gr. of each metal, dry and warm, being mixed with 100 gr. of the salt likewise newly heated. To obviate any fallacy from common sal ammoniac being employed, I repeated the experiment with the salt formed from the combination of its two constituent gases, and obtained the same result. But although this affords an easy mode of exhibiting the production of water, it is not favourable to obtaining a perfect result, the heated ammoniacal gas carrying off a considerable portion of the water deposited; and accordingly the quantity, instead of increasing as the experiment proceeds, at length diminishes, and the ammoniacal gas deposits a portion of water in passing through mercury, or in being conveyed through a cold tube.

But though I consider this conclusion as established, there is a considerable difficulty attending the theory of the experiment. The result of water being obtained is actually different from what is to be looked for on the doctrine of muriatic acid gas containing combined water; and even when the fact is established, the theory of it is not easily assigned. On that doctrine, it must be held that in the action of metals on muriatic acid gas, the metal attracts oxygen from the water, the corresponding hydrogen is evolved, and the oxide formed combines with the real acid. No water, therefore, ought to be deposited; for none is abstracted from the acid but what is spent in the oxidation of the metal. This will be apparent by attending to the proportions in a single example from the scale of chemical equivalents: 100 gr. of iron combine with 29 of oxygen, and in this state of oxidation unite with 99 of real muriatic acid. This quantity of acid exists in 131·8 of muriatic acid gas combined with 32·8 of water; and this portion of water contains 29 of oxygen with 3·8 of hydrogen. There is present, therefore, exactly the quantity of oxygen which the metal requires to combine with the acid; and no water remains above this; or it may be illustrated under another point of view. Muriatic acid gas is composed of oxymuriatic gas and hydrogen. A metal acting on it must attract the oxymuriatic acid—that is, the muriatic acid and oxygen, and liberate the hydrogen. No water, therefore, ought to appear more on this theory than on the other; but the real products in both must be a dry muriate, or chloride, and hydrogen gas. In the action of ignited metals on muriate of ammonia, it is equally evident, on the same principle, that no water ought to be obtained. How then is the production of water to be accounted for?

Though the water obtained in these experiments cannot be derived from hygrometric vapour in the gas, there is another view under which it may be regarded at present as an adventitious ingredient. The acid having a strong attraction to water, may be supposed in the processes in which it is usually prepared, to retain a portion not strictly essential to its constitution as muriatic acid gas, but still chemically combined—that is, combined with it with such an attraction as to be liberated only when it passes into other combinations, and it may be this portion which is obtained in the action of metals on the gas; the other portion, that essential to the acid, being sufficient to produce the requisite oxidation of the metal.

The question with regard to the existence of water in this state, Gay-Lussac and Thenard have already determined. From an extensive series of experiments, they found reason to conclude, that muriatic acid gas, in whatever mode it is prepared, is uniformly the same. From the quantity of hydrogen gas which combines with oxymuriatic gas in its formation, it follows that it contains 0·25 of water essential to its constitution. But the gas obtained by the usual processes, afforded, they found, exactly 0·25 of water, when transmitted over oxide of lead, or combined with oxide of silver; and the same compounds are formed as by the action of oxymuriatic acid on silver and lead in their metallic state. They prepared muriatic acid gas, by heating fused muriate of silver with charcoal moderately calcined. It contained just the same quantity of water as muriatic acid obtained from humid materials, as it afforded the same quantity of hydrogen from the action of potassium. And instead of being capable of receiving the smallest additional portion of water, a single drop of water being introduced into three quarts of it, did not disappear, nor even diminish; but, on the contrary, increased in volume.* These facts establish the conclusion, that muriatic acid gas can receive no additional portion of water but that which is essential to it, and hence preclude the solution of the difficulty under consideration by the opposite assumption. And it is to be remarked, that should even such a portion of water exist in the gas, it cannot be supposed that the acid should carry this with it into its saline combinations, and retain it so that it should not be expelled by heat. It cannot be supposed to exist, therefore, in muriate of ammonia thus heated, and, of course, cannot account for the water obtained by the action of the metals on this salt.

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* Recherches Physico-chimiques, tom. ii. p. 133.

When it is proved that no extrinsic water exists in muriatic acid gas, there remain apparently only two modes on which the production of water can be explained, either that the metal may require less oxygen than is supposed in combining with the acid, so that a portion of water will remain undecomposed to be deposited; or that the oxide attracts more real acid, so as to liberate a larger proportion of water. The first of these suppositions is improbable, from the consideration of the law which regulates the combination of metallic oxides with acids; that the quantity of acid is proportional to the quantity of oxygen, so that if an oxide were formed in these cases at a lower degree of oxidation, it would only combine with a proportionally smaller quantity of acid, and the quantity of water detached from the combination would be the same.

No improbability is attached to the second supposition; and it has even some support from the consideration that many metallic saline compounds form with an excess of acid, and that it is difficult, with regard to a number of them, to procure them neutral. Metallic muriates, with excess of acid, seem in particular to be established with facility. And although an excess of metal be present in the action exerted on muriatic acid gas, this may not prevent the formation of a super-muriate, more especially as the excess is in the metallic form, and exerts no direct action, therefore, on the real acid.

To ascertain if a super-muriate were formed in these cases, the product obtained from the action of the muriatic acid on the metal was raised to a heat as high as could be applied without volatilization, so that no loosely adhering acid might remain, and the air in the retort was repeatedly drawn out by a caoutchouc bottle. The solution from the residue both of iron and zinc was very sensibly acid. Some fallacy, however, attends this, from the circumstance that the liquid state is necessary to admit of the indications of acidity; and in adding water to produce this, a change occurs in the state of combination in a number of the metallic muriates; a supermuriate being formed which remains in solution, and a submuriate being precipitated, so that the acidity of the entire compound cannot justly be inferred from that of the solution. I found accordingly, that on adding water to the product from the action of the acid gas on zinc this change occurs; a little of a white precipitate being thrown down, while the liquor remained acid. But the fallacy can be obviated, by adding only as much water as produces fluidity without subverting the combination. Portions, therefore, of the residue were exposed to a humid atmosphere, until by deliquescence liquors were formed transparent without any precipitation; and these were strongly acid, reddening litmus paper when it was perfectly dry and warm. I further found that the product of the solution of zinc in liquid muriatic acid, when digested with an excess of metal and evaporated to dryness, afforded by deliquescence a liquor sensibly acid; and in both cases, even when the solid product was retained liquid by heat, acidity was indicated by litmus paper. Lastly, what is still less liable to objection, the residue in the experiment of heating the muriate of ammonia with the different metals, afforded similar indications of acidity.

These results appear to establish the production of a supermuriate in the action of these metals on the acid, and this accounts for the appearance of a portion of water, since, supposing water to exist in muriatic acid gas, the quantity combined with that proportion of acid which would establish a neutral compound is the quantity required to oxidate the metal to form that compound; and if any additional portion of acid enter into union, the water of this must be liberated, or be at least capable of being expelled.

It was of importance, in relation to this question, to ascertain the quantity of hydrogen obtained from a given quantity of muriatic acid gas; for if the whole water essential to the acid is decomposed by the action of the metal, half the volume of hydrogen ought to be obtained, muriatic acid gas being composed of equal volumes of oxymuriatic gas and hydrogen gas. I made this repeatedly the subject of experiment by heating zinc and iron in muriatic acid gas. There are difficulties in determining the proportion with perfect precision; but the quantity of hydrogen always appeared to be less than the half; and on an average, about 12 measures were obtained, when 30 measures of the other had been consumed, a result conformable to the liberation of a portion of the combined water of the gas.

Whether the production of water in these experiments is satisfactorily accounted for on the cause now assigned, may be subject of further investigation. In the sequel I shall have to notice another principle, on which, perhaps, it may fall to be explained. Whether accounted for or not, it is obvious that the fact itself is not invalidated by the theoretical difficulty; and also, that in relation to the argument with regard to the nature of muriatic and oxymuriatic acids, it remains equally conclusive. In the doctrine of the undecomposed nature of chlorine, muriatic acid gas contains neither water nor oxygen, and the metal employed certainly contains none. These are the only substances brought into action, and it is impossible that water should be a product of their operation. On the opposite doctrine, water is held to exist in muriatic acid gas to the amount of $\frac{1}{4}$th of its weight; and it is conceivable that by some exertion of affinities, a portion of it may be liberated. If we were unable to explain the *modus operandi*, this would remain a difficulty no doubt, but not, as in the opposite system, an impossible result.

It is to be admitted, indeed, that in none of these cases is the entire quantity of water which must be supposed to exist in muriatic acid gas obtained; and so far the proof is deficient. But neither from the nature of the experiments is this to be looked for; and I give more weight to the argument from having always found certain portions of water to be procured; while on the opposite doctrine there should be none. In those cases where, supposing water to be present in muriatic acid gas, it ought to be obtained in the full quantity, it uniformly is so, though the proof from these is rendered ambiguous by the result being capable of being explained on a different hypothesis.

(*To be continued.*)

Article VI.

Contributions towards the History of Anthrazothionic Acid, discovered by Porrett, and called by him Sulphuretted Chyazic Acid. By Theodor von Grotthuss.*

Sect. 1. *Choice of the Name.*—The name which Porrett has given to this acid is formed from the first letters of the names of the elements of which it is composed, namely, carbon, hydrogen, and azote. As the object undoubtedly must have been to bring to the recollection those constituents of the acid about which no doubt exists; and as the name given by Porrett was unsuitable to the idiom of all other languages except the English, it was natural to endeavour to correct the want of euphony of the term by alterations which should still recall the elements of which the acid is composed. In Germany accordingly the term *sulphuretted prussic acid* (*schwefel-blausäure*) was pitched upon; but this name is admissible only on the supposition that Porrett's assertion that this acid is a compound of prussic acid and sulphur be true. But from the experiments which I am going to relate, it will be seen that it contains indeed the elements of prussic acid, but in different proportions; and that neither *prussic acid* nor *cyanogen* as such exist in it. Hence the last term cannot be applied to it with more propriety than the first term.

I resolved, therefore, instead of Porrett's name, to compose the term *anthrazothionic acid* (*anthrazothionsäure*) from the Greek, indicating accurately the constituents of which this acid is composed; and I hope that the term will be adopted by chemists. It is composed of abbreviations of the Greek words *ανθραξ, charcoal, αζώη, azote,* and *θειον, sulphur.* As this acid contains no oxygen, but hydrogen (as is sufficiently established by Porrett's experiments); and as it appears from my experiments and from those of Porrett that when exposed to the action of the Voltaic battery its sulphur separates at the positive pole, but its hydrogen with its other constituents makes its escape in a gaseous form at the negative pole, it follows that it must be considered as a *hydracid.* With respect to the name *hydracid,* two remarks must be made. 1. In the hydracids, and consequently in anthrazothionic acid, it has been shown that the hydrogen acts the same part that oxygen does in other acids, and that it is substituted for it. 2. At present no other acid is known composed of *anthrazothion* (carbon, azote, and sulphur) and oxygen. I have not been able indeed to obtain the principle, to which I give the name of *anthrazothion,* in an isolated state; but I conclude from analogy with cyanogen, and from various other reasons to be stated below, that it exists at least in a state of combination. From this non-existence of a combination of anthrazothion and oxygen, I conclude that the principle is not liable to change its state. By the term *anthrazothion,* I mean anthrazothionic acid deprived of its hydrogen. When this acid comes in contact with easily reducible metallic oxides, its hydrogen combines with the oxygen of the oxide, and forms water, while the anthrazothion forms a combination with the reduced metal.

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* Translated from Schweigger's Journal, xx, 225. (Published in Dec. 1817.)

I may remark here by the way, that those acids whose radical becomes acid as well by uniting with oxygen as with hydrogen, may be very easily distinguished from each other by the electro-chemical properties of the radical. Thus, for example, sulphuric acid might be called *schwefelplussäure* (*sulphur plus acid*), and sulphuretted hydrogen *schwefelminussäure* (*sulphur minus acid*). By this the cacophony of the frequent repetition of the syllable *stoff* would be avoided.*

Sect. 2. *Preparation and Properties of Anthrazothionate of Potash.*—Porrett has described different methods of forming this salt, which may be seen in his paper. I conceive that it will not be superfluous to give an account of the way by which I procured it in a state of purity.

One part of prussiate of potash in the state of dry crystals was rubbed to a fine powder with the third part of its weight of sulphur, and the mixture beaten down firm into a crucible, in the bottom of which a little sulphur had been previously put. This crucible was put into the fire, covered, raised to a red heat, and allowed to remain at that temperature for half an hour or longer. From the very first, a peculiar penetrating substance was disengaged, having a peculiar smell (which is not similar to that of prussic acid). When a burning body is brought into contact with this substance, it takes fire, and burns with a light white flame mixed with blue, which may be extinguished by again putting the cover over the crucible. This gas is either the *cyanogen* discovered by Gay-Lussac, or *prussic acid;* for if a paper moistened with liquid ammonia be held over it for a while, it gives with an acid solution of oxide of iron excellent prussian blue; but I have not made any more accurate experiments on it. The mass in the crucible concretes together, melts, assumes an appearance very similar to graphite, and shows here and there, particularly along the fracture, a quantity of small metallic specks, similar to iron with a silvery lustre. When at this period, the fire is raised, by blowing to a white heat there comes over at last another gas, the bubbles of which as soon as they pass through the melted graphite-looking matter take fire in the air of their own accord. A white flame and weak explosion characterize this combustion. If a polished plate of steel be held over the crucible at this period, it becomes covered with subtile flocks of a whitish grey colour, which act as an alkali upon turmeric paper. As the gas is always distinguished by the peculiar smell already mentioned, and as it always forms prussian blue when treated with liquid ammonia and an acid solution of oxide of iron, it must consist of cyanogen mixed with potash, or of prussic acid gas containing potash.*

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* It is obvious that the preceding section relates entirely to the names in the German language. I found some difficulty in making it intelligible to the English reader, and am not sure that I have succeeded.—T.

Sect. 3.—The black, graphite looking matter in the crucible is now allowed to cool, it is taken out, rubbed down to powder, and set to digest in alcohol. After some time, the liquid is passed through the filter, and a new portion of alcohol poured upon the powder. This digestion of new alcohol is continued till the liquid ceases to alter the colour of solutions of iron. The alcoholic solution is usually colourless and transparent, though sometimes it has a blood red colour, owing to the presence of a portion of anthrazothionate of iron, from which it is easy to free it by the addition of a few drops of the alcoholic solution of potash. The oxide of iron falls down, the red colour disappears, and the liquid when filtered becomes quite colourless. It contains pure *anthrazothionate of potash*, which by gentle evaporation is obtained in long channelled prisms terminated by four-sided pyramids. It frequently crystallizes likewise in long, white, brilliant needles; it is much more soluble in boiling than in cold alcohol. Hence it frequently happens when a glass is half filled with a boiling hot saturated solution of this alcohol, that the salt during the cooling shoots up from the bottom of the glass in brilliant needles, the ends of which, in consequence of the adhesion of the liquid and its universal contraction, are some lines higher than the surface of the solution.

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* When the experiment is repeated in proper metallic tubes (which better keep off the action of atmospherical oxygen), there can be no doubt that potassium will be obtained at a lower temperature, and with greater ease, than by the method hitherto practised. Ought not this gas to be formed when potassium is heated in cyanogen? And ought not the hydrogen gas, which remains behind when the excess of cyanogen is absorbed by potash, to be ascribed to the decomposition of the water which the potassium in the cyanogen finds in the potash? Gay-Lussac found that when 48 or 50 volumes of cyanogen were combined with potassium, the remaining cyanogen, after it had been absorbed by potash (which always contains moisture), left a residuum amounting to 12 parts. But this is an equivalent to 24 volumes of hydrocyanic acid: hence it follows that the cyanogen under examination must have contained the third of its volume of hydrocyanic acid. When we consider Gay-Lussac's accuracy, and the excellent apparatus and reagents which he had at his disposal, so great an impurity in the cyanogen prepared by him, and contrary to his wishes, is very unlikely.

Sect. 4.—Anthrazothionate of potash has at first a hot taste, similar to that of radishes, but leaves in the mouth a cooling salt impression. In summer, when placed in dry air, it retains its crystalline form unaltered; but as soon as the air becomes in the least moist, for example, a little before the dew begins to fall, at six in the evening, it becomes liquid, and remains in that state till ten o'clock the following morning. When it has not become solid by that time, or at the latest by 11 o'clock, it is a proof that the atmosphere is moist; and we may predict with some probability that it will rain either on that day or the following one. For such an experiment the salt must not be exposed to the sunshine, but placed in the open air in a watch glass, and in a place moderately shaded. The remarkable delicacy with which this salt indicates the hygrometrical changes of the atmosphere, the readiness with which it gives out the water which it has absorbed when put into a dry place, fit it, I think, particularly for hygroscopical investigations; and if it were placed upon a scale properly balanced, it would form an excellent hygrometer. When exposed to heat in a glass tube, it melts quietly into a colourless and glass-looking mass, which on cooling congeals again in crystals. It is capable of bearing a much higher temperature without decomposition than prussiate of potash. However, when it is exposed to a red heat in certain metallic vessels, for example in silver, sulphuret of silver is formed which blackens the metal, and at the same time ammonia is produced.

Sect 5.—*Diluted sulphuric acid* drives off the anthrazothionic acid undecomposed. We may, therefore, by its means easily obtain this acid in a free state in a proper receiver. The tedious and complicated method, therefore, which Porrett has given for obtaining it is quite unnecessary. *Concentrated sulphuric acid* drives off only a part of the acid undecomposed from the anthrazothionate of potash. The remainder undergoes a decomposition: an effervescence takes place; the sulphur separates in flocks, while carbonic acid gas and sulphurous acid gas fly off. But the most remarkable circumstance (which Porrett seems to have entirely overlooked) is that during this decomposition, which goes on rapidly when assisted by heat, *no trace of azotic gas* *appears, but the whole of it is converted into ammonia, which unites with the sulphuric acid.* The ammonia may be detected by pouring an excess of concentrated potash ley upon the liquid after the chemical action is at an end. A very strong smell of ammonia immediately becomes sensible, especially if heat be applied. This observation, as will be seen below, was of great importance to me in forming a true notion of the nature of anthrazothionic acid.

Sect. 6.—Common concentrated muriatic acid likewise separates the acid from the salt without decomposing it; but it is not quite pure, being mixed with muriatic acid. *Concentrated liquid chlorine* prepared by pouring concentrated muriatic acid upon chlorate of potash decomposes the salt quite in the same way as concentrated sulphuric acid: the sulphur is precipitated in yellow flocks; but when the salt is mixed in the first place with chlorate of potash, and then muriatic acid poured over it, no sulphur separates, but it is completely converted into sulphuric acid, while the anthrazothionic acid is completely decomposed. At the same time muriate of ammonia is formed and carbonic acid is disengaged.

Smoking nitric acid when poured upon the salt occasions a violent effervescence, nitrous gas and carbonic acid are given off, while the sulphur separates in substance.

Some vegetable acids, as, for example, tartaric acid, are capable of separating the anthrazothionic acid from the salt, especially when assisted by heat.

Sect. 7.—In no one of the decompositions performed by means of the different mineral acids, though they were often repeated by me, have I been able to perceive so much as a trace either of *prussic acid* or of *cyanogen* in a free state, though I examined both the liquids that came over into the receiver and those which remained behind in the retort. No prussian blue was ever formed when they were treated with ammonia and an acid solution of oxide of iron. Hence I consider myself as entitled to conclude with the greatest certainty that *neither cyanogen nor prussic acid as such are elements of anthrazothionic acid*. And this conclusion is not only confirmed in the fullest manner by the decomposition of the acid by means of the galvanic battery; but also by the decompositions of the various combinations of anthrazothionic acid by means of heat to be related below. As Porrett affirms the contrary of this, there can be no doubt that he must have operated upon a salt containing a prussiate mixed with it. This would be more readily the case as he employed no alcohol, and indeed no method whatever to free anthrazothionate of potash from the prussiates.

Sect. 8.—There are much stronger grounds for concluding from what has been already stated that *ammonia* is one of the immediate constituents of anthrazothionic acid; paradoxical as it may seem that an *alkali* should constitute *an* element of an acid; for in all cases in which this acid was decomposed by means of sulphuric acid, or chlorine, I have been able at last to detect the presence of ammonia by means of potash ley. Strong white clouds made their appearance, when a feather dipped in acetic acid was held over the liquid, and the ammonia at last became sensible by its smell. We might, perhaps, conclude, that when the anthrazothionic acid was decomposed, the *water* of the acid employed to decompose it (chlorine or sulphuric acid) supplied the hydrogen for the formation of the ammonia by uniting with the azote of the anthrazothionic acid, while the oxygen of the water might unite with the carbon of that acid and form carbonic acid; but in that case *common muriatic acid* must be capable of producing the same decomposition. Now as this is not the case, I conclude that the hydrogen which goes to the formation of ammonia during the decomposition cannot have been furnished by the water; but must undoubtedly have *existed in the anthrazothionic acid itself together with the azote in the very proportion adapted for the formation of ammonia.*

Sect. 9.—If a concentrated solution of anthrazothionate of potash in water be exposed to the action of a good Voltaic battery, a great evolution of gas takes place at the negative pole. This gas has a peculiar smell, similar to that of the inflammable gas from marshes. It is itself combustible, and when burned, carbonic acid gas is formed; and there remains a residuum of azotic gas. I consider this gas as a triple compound of carbon, hydrogen, and azote; though indeed it may be only a mixture of carburetted hydrogen and azotic gas. At the positive pole no gas is extricated; the liquid from the sulphur contained in it becomes yellowish, and at last allows the greater part of the sulphur to precipitate in large flocks. If silver or any other easily sulphuretted metal be placed in contact with the positive pole, it becomes immediately black by entering into combination with the sulphur; at the negative pole it remains completely white. It is possible that it may form a hydrate there, though the supposition is not very probable. The liquid, after having been exposed for some hours to the action of a battery of 100 pair of round plates, nine inches in diameter, was tried at both poles to discover in it the presence of prussic acid, nitric acid, and ammonia; but not a trace of one of these bodies could be found. The liquid from both poles, when mixed with a solution of iron in an acid, struck a blood-red colour, which must be ascribed to the presence of undecomposed anthrazothionate of potash. The sulphur at the beginning of the process remained in solution at the positive pole, and gave the liquor a yellow colour. This shows us that anthrazothionic acid is capable of existing with two proportions of sulphur, namely, a *minimum*, and a *maximum*. In this last state it must form peculiar compounds with the bases, which deserve hereafter to be accurately examined. If, as Porrett supposes, the acid were a compound of prussic acid (or cyanogen) and sulphur, the former of these constituents would have appeared in abundance at the negative pole; and the liquid from that pole when mixed with a solution of iron and an acid would have formed a great deal of prussian blue.

Sect. 10.—One part of dry anthrazothionate of potash being mixed with five parts of chlorate of potash was inflamed by friction and percussion. Concentrated sulphuric acid inflamed the mixture still more violently; and when the experiment was attempted in a glass tube, a dangerous detonation took place. The copper anthrazothionhydrate, considered by Porrett as an anthrazothionate of copper, exhibits the same phenomena as Porrett has remarked. I was in hopes in this way to have been able to have collected the azote from this last compound, and to have determined its quantity in a glass tube over mercury; but the instant the sulphuric acid came in contact with a quarter of a grain of hydrate and 1¼ gr. of chlorate of potash, a violent detonation took place, the tube was broken to pieces in my hand, and I received some slight wounds from the fragments of the glass scattered about in all directions. On another occasion, the same mixture took fire while I was rubbing it slightly in an agate mortar.

Sect. 11. *Metallic Anthrazothionhydrates.*—I consider all those bulky insoluble precipitates which take place when a solution of anthrazothionate of potash is poured into a solution of an easily reducible metal, as compounds in which the metal exists in the metallic state united with the anthrazothionic acid deprived of its hydrogen, that is to say, with *anthrazothion*, and with the water formed by the union of the hydrogen of the acid with the oxygen of the oxide, constituting a *metallic anthrazothionhydrate.* The following observations exhibit the grounds of this opinion. 1. When this precipitate is heated in a glass tube, after having been dried for a day in a temperature between 122° and 144°, it always lets go a notable quantity of water, which collects in the cool part of the tube in the state of drops. 2. This water corresponds (at least I have found this to be the case in the copper anthrazothionhydrate) to the *sum of the oxygen in the oxide and of the hydrogen in the acid.* 3. During the escape of the water, which takes place at a temperature considerably under a red heat, the colour of the substance distinctly alters and becomes dark: at the same time there separates a peculiar gaseous body with a particular smell, which I, both from the analogy of cyanogen, and because *it is absorbed by ammonia and then strikes a blood-red colour with a solution of iron*, consider as *anthrazothion.*[*]

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[*] I regret very much that I have not been able to bring forward more direct proofs of the existence of anthrazothion, as no method which I attempted succeeded with me in separating it from the metallic anthrazothion compounds and procuring it in a separate state. Porrett must, in his experiments also, have employed only a very small quantity of this substance. It is much easier to procure cyanogen in

The greatest part of the sulphur unites itself with the metal forming a sulphuret; azote, or rather carburetted azotic gas, is given off, a portion of the carbon remains long behind, and by the increase of heat and the free admission of air it glows like a pyrophorus, a little before the temperature rises to redness. After this pyrophoric appearance, but not before, sulphate of copper may be washed out of copper anthrazothionhydrate. 4. The metallic sulphuret which remains behind after the hydrate has been exposed to a red heat without the access of air, contains the metal *in the metallic state.* Nitric acid dissolves it when assisted by heat with the evolution of abundance of nitrous gas, and leaves the sulphur behind. 5. The copper anthrazothionhydrate is not sensibly attacked by concentrated muriatic acid even at a boiling temperature, provided care be taken not to allow any oxidizing substance to come in contact with it; but if a little of any such substance, for example, of chlorate of potash, be added, the copper is oxidized, and the anthrazothion decomposed. If this muriatic solution be evaporated to dryness, and the dry mass mixed with potash ley, a strong smell of ammonia becomes perceptible. Now if the metal in this compound were in the state of an oxide, concentrated muriatic acid would surely be capable of separating the acid from the oxide, as it does from all the alkaline anthrazothionates, and even with much greater facility; but this, however, is not the case. The muriatic acid is never able to dissolve the metal till an oxidizing body comes into play and gives out its oxygen to the metal. 6. During the combination of anthrazothionic acid with easily reducible metals, the former must undoubtedly reduce the latter, because three of its elements, namely, hydrogen, sulphur, carbon, are capable of reducing not only these, but many others to that state; and the fourth element, azote, is at least neutral, if it does not rather promote disoxidation. Ought not then the hydrogen, the most disoixdizing of all the elements, be capable of reducing these oxides to the metallic state, since the water thereby produced may combine with the metallic anthrazothion (or *anthrazothionide,* if that name be preferred) and form a hydrate? 7. To show in the last place that the opinion which I entertain respecting these compounds is at least not more hypothetical than Porrett's, I may observe that hitherto nobody has been capable of deciding certainly about the way in which the elements are united in a compound. Nobody can, for example, in this case show that the hydrogen of the acid does not unite with the oxygen of the oxide (which from the preceding observations is highly probable); but that the former remains in the acid, and the latter in the oxide, after these two have united together and constituted a new body. In this point of view

quantities than anthrazothion; because some of the cyanogen compounds are prepared on a large scale, and may be had in quantities, while no anthrazothion compounds are to be met with in apothecaries' shops.

both opinions appear equally valid; while the former, from the reason stated, and from the properties of the metallic anthrazothionhydrates, seems to me much more likely to be the true one.

Sect. 12. *Properties of some metallic Anthrazothionhydrates.* —*a.* That of *silver* is white, curdy, and very voluminous; it has some resemblance to muriate of silver, and becomes black by exposure to the atmosphere, at least if it has been treated with ammonia and well washed; but it does not become so black as muriate of silver; from which it is very easily distinguished, as it is not soluble in ammonia. In this respect it resembles iodide of silver, with which, however, it cannot be compared in its other properties. Liquid chlorine forms with it muriate of silver, sulphur is precipitated, ammonia formed, and carbonic acid gas evolved. Chlorine produces the same effect upon all the other metallic anthrazothionhydrates.

b. The anthrazothionhydrate of *gold* is capable of assuming different colours, according to the way that it is prepared. When treated with muriatic acid and water, it becomes gradually of a dark purple colour. When the dark precipitate is put into a concentrated solution of anthrazothionate of potash, it assumes a light flesh colour, doubtless because it enters into combination with a little of the anthrazothionate of potash. Should not the analogy of the formation of white prussiate of iron be attended to here? When the flesh coloured substance is put into muriatic acid, it becomes dark purple. Potash deprives it of a part of its anthrazothion, and gives it a yellow colour. The precipitate when first formed, and even after being dried, is very voluminous. The anthrazothionide of gold is soluble in liquid anthrazothionate of potash, and the solution has a dark red colour. I added an excess of anthrazothionate of potash to a neutral solution of gold in muriatic acid, and then filtered the liquid; it passed through the filter dark red. Some drops of ammonia threw down a black powder from this liquid, and the dark red colour disappeared.

c. The anthrazothionhydrate of *mercury* is white and bulky, and is formed by double decomposition only when the metal is in solution in an acid in the state of protoxide. The solution of corrosive sublimate is not precipitated by anthrazothionate of potash; but if a piece of tin be put into the mixture, the anthrazothionhydrate of mercury is precipitated mixed with metallic mercury. It would seem that when the metal of an anthrazothionhydrate contains more oxygen than the hydrogen of the acid is capable of taking up, and when in this state it is in solution in an acid, it is capable of forming with anthrazothionic acid by means of double decomposition a soluble *anthrazothionate*, but not an *anthrazothionhydrate.*

d. The anthrazothionhydrate of platinum is yellowish, bulky, and easily soluble both in acids and in liquid muriates. From these last solutions, alcohol throws down the hydrate in yellowish white flocks.*

The alkalies and the non-oxidizing acids, when no oxygen can interfere, appear incapable of producing any effect upon the metallic anthrazothionhydrates.

Sect. 13. *Anthrazothionate of Iron.*—This compound when seen by transmitted light appears blood-red; by reflected light it appears quite black, though now and then it shows a dark green metallic lustre on the surface. It deliquesces in the air, and cannot be obtained in the state of crystals. Acids deprive it of its iron oxide, and alkalies of its acid. In both cases the peculiar colour which distinguishes it disappears. It is exceedingly soluble in absolute alcohol, which enables us to obtain it in a state of great purity; it has a disagreeable, styptic, metallic taste. This property which anthrazothionic acid has of striking a strong red colour with oxide of iron renders the anthrazothionate of potash a very useful reagent for detecting the presence of that metal. It is incomparably a more delicate test than prussiate of potash, though not quite so delicate as the infusion of nut-galls. When an alkaline carbonate is present in a mineral water together with carbonate of iron, as is the case in *Gelenauer water*, in that case neither anthrazothionate of potash nor prussiate of potash is capable of detecting the presence of the iron; we must, therefore, in the first place, neutralize the carbonate by means of a stronger acid; then the liquid will strike a red colour with anthrazothionate of potash. The red colour of anthrazothionate of iron, when applied to organized bodies, as skins, paper, linen, wool, silk, is very fugitive, because the acid gradually makes its escape. Perhaps it might be fixed by means of a mordant.

Sect. 14. *Constituents of Anthrazothionhydrate of Copper.*—This compound is formed when anthrazothionate of potash is mixed with a solution of copper and a disoxidizing body, as, for example, sulphate of iron is added to the mixture. From the origin of this white, bulky precipitate, described by Porrett, it seems to follow that the hydrogen in the acid is not sufficient to convert the whole oxygen of the oxide into water. Hence the reason why the assistance of a disoxidizing substance is necessary for the formation of the hydrate. Porrett considers this compound as an anthrazothionate of copper, and states its constituents at 36·855 acid and 63·145 oxide of copper. But it contains a notable quantity of water, though Porrett affirms the contrary, even when it has been long dried in as high a temperature as it can be exposed to without altering its *white* colour (which would indicate a decomposition); for when it is heated in a glass tube, large drops of water are deposited in the cool part of the tube. We must, therefore, consider it as a hydrate; and the proportions of the constituents as given by Porrett require to be altered.

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* The properties of the anthrazothionhydrate of copper have been already described by Porrett.

Three grains of the hydrate being exposed to heat till they became dark coloured gave out $\frac{3}{8}$ gr. of water. Hence it follows that $\frac{1}{8}$ of the weight of this hydrate is water.

It is obvious likewise that the metal must exist in the hydrate in the metallic state, since the hydrogen of the acid *reduces* the oxide, a fact which can be evidently observed even during the formation of this hydrate; for when the alcoholic solution of anthrazothionate of potash is mixed with liquid acetate of copper, we can perceive at the instant of the mixture a *brown copper colour* upon the surface of the liquid, which disappears after the hydrate is completely formed. In this case either the alcohol or the acetic acid must act the part of a disoxidizing body. The necessity of the presence of this intermediate substance shows that the hydrate can be formed only by means of the sum of the affinities of the oxygen for the hydrogen, and of the anthrazothion for the metal.

Sect. 15.—As we can employ as disoxygenizing substances bodies which possess that property in a far smaller degree than is necessary to change the peroxide of copper into protoxide, it follows as a consequence that these disoxygenizing substances actually separate from the oxide much less oxygen than would be requisite in order to convert it into protoxide. The remaining part of the task is performed by the hydrogen of the anthrazothionic acid. We will assume, therefore, that the disoxygenizing medium deprives the oxide of $\frac{1}{5}$th of its oxygen, while the remaining $\frac{4}{5}$ths unite themselves to the hydrogen of the acid at the same instant that the metal combines with the anthrazothion, and with the water produced forming an anthrazothionhydrate of copper. The accuracy of this assumption will still further appear from this, that we shall find in quite another way exactly as much hydrogen in the acid of the hydrate as is sufficient for saturating $\frac{4}{5}$ths of the oxygen, which the oxide contains. I may mention as a second corroboration of the truth of this assumption, that the hydrate contains exactly as much water as is capable of being formed by the union of $\frac{4}{5}$ths of the oxygen of the oxide with the hydrogen of the acid; namely, $\frac{1}{8}$th of the weight of the whole compound as was shown in the last paragraph.

Sect. 16.—Porrett found that 4·58 gr. of anthrazothionhydrate of copper contain 2·56 gr. of metal, which require 0·64 gr. oxygen to be converted into oxide. According to our view of the subject, $\frac{1}{5}$th of this oxygen = 0·128 gr. unites with the disoxygenizing medium. There remain, therefore, $\frac{4}{5}$ths of 0·64 = 0·512 gr. of the oxygen, which unite with the hydrogen of the anthrazothionic acid to form water. When, therefore, 2·56 gr. metal + 0·512 gr. oxygen = 3·07 gr. is subtracted from the sum total of hydrate, we obtain the quantity of acid; but 4·58 − 3·07 = 1·51, which must be the amount of the acid. These 1·51 gr. of acid must contain 0·067 hydrogen, because this is the quantity requisite to convert 0·512 gr. of oxygen into 0·578 gr. of water. Hence it follows that 4·580 gr. of anthrazothionhydrate of copper contain 2·56 gr. of copper, 1·510 gr. of anthrazothionic acid, and 0·512 gr. of oxygen, or according to the accurate way of viewing this compound, that not the acid but the anthrazothion is united with the metal and with the water, the constituents are,

Copper	2·560	
Anthrazothion	1·442	(= 1·510 acid − 0·067 hydrogen)
Water	0·578	(= 0·511 oxygen + 0·067 hydrogen)
	4·580	

or in 100 parts,

Copper	55·89
Anthrazothion	31·48
Water	12·63
	100·00

(*To be continued.*)

ARTICLE VII.

A Method of separating Iron from Manganese. By Theodor von Grotthuss.*

DISSOLVE the metal, or the oxide, in muriatic acid; and as it is necessary to convert the iron into a peroxide, pour a few drops of nitric acid into the solution, and evaporate it till it becomes doughy and merely moist. Pour over it, when in this state, a solution of anthrazothioniate of potash in alcohol, and mix the whole well together. The liquid becomes immediately of a blood red colour, because anthrazothionate of iron is formed which dissolves in the alcohol. The anthrazothionate of manganese falls down in the state of a white powder, because it is not soluble in absolute alcohol. Add a portion of alcohol, filter the liquid, and wash the white insoluble anthrazothionate of manganese repeatedly with small portions of alcohol. It will be manifest that the manganese is quite freed from all admixture of iron when the alcohol comes off from it quite colourless. The oxide of iron may be precipitated from the red liquid by means of potash ley, and its quantity ascertained. Dissolve the anthrazothionate of manganese in water, and precipitate the manganese by means of potash. From the oxides thus obtained, we may determine the quantity of metal by Berzelius's method; but this method of separating the two metals is not absolutely correct, because anthrazothionate of manganese is *slightly soluble* in alcohol.

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* Translated from Schweigger's Journal, xx. 272.

ARTICLE VIII.

A remarkable Combination of Carbonate of Lime and Hydrate of Lime, observed by Theodor von Grotthuss.*

WHEN a strong current of carbonic acid gas is suddenly passed through lime water, there is formed not a pure *carbonate of lime*, but a mixture of *carbonate and hydrate*. This fact deserves attention, because in many cases it is easy to mistake one of these compounds for the other. It is very bulky, and falls slowly to the bottom in flocks; but it has only an ephemeral existence; for as soon as these flocks approach near each other, they lose at once their voluminous appearance, and do not assume it again when agitated. They have now a granular, powdery consistence, and a much greater specific gravity, in consequence of which they sink rapidly to the bottom, and do not appear in flocks. The substance in this last state is pure carbonate of lime. If concentrated acetic acid be poured upon the bulky precipitate when first formed, not the smallest evolution of air bubbles is perceptible; because the carbonic acid as it is set at liberty, finds, in consequence of the great bulk of the hydrate and of the water which it contains, so many points of contact with it, that it cannot assume the gaseous form. If a concentrated solution of an ammoniacal salt, for example, sal ammoniac, be poured into the bulky hydrate, which renders the water as white as milk, the liquid becomes immediately almost colourless, because the hydrous carbonate passes at once into the state of pure carbonate, which last has a very small volume when compared with the former. Were the bulky precipitate merely a hydrous carbonate, we might suppose that the salt deprives it of its water; but a concentrated solution of common salt does not, by any means, produce the same appearance. The bulky precipitate, therefore, must be a compound of carbonate and hydrate of lime. The hydrate unites with the acid of the ammoniacal salt and sets the ammonia free, while the pure carbonate only remains behind. The action of the water and attraction of cohesion of the carbonate of lime, appear gradually to destroy the compound. It has, therefore, as has been already observed, only an ephemeral existence. It is very easy to form it, merely by blowing out air from the lungs through a glass tube in lime water. Carbonate of magnesia seems to form the same kind of combination; for, according to Bucholz, it is capable of existing in the state of three different compounds.

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* Translated from Schweigger's Journal, xx. 275.

Article IX.

Description of an improved Microscope. (See Pl. LXXXVIII.)

Fig. 1. The instrument mounted for viewing an *opaque* object. A B is the body of the microscope; it consists of five lenses, and differs from that commonly used for microscopes. The instrument-maker will comprehend how to make it when he is told that the lenses 1 and 2 at the bottom are similar to those used for the eye-pieces of refracting telescopes; the lenses 3, 4, and 5, are the field-glass and double eye-glass used in compound microscopes. To increase the magnifying power, there are three astronomical eye-pieces of different powers, which are made to fix on at A. This body is eight inches long, the bottom is tapered a little, as may be seen at B, for the purpose of allowing the rays to pass more freely from the mirror S to the object to be examined. At the top, A, a shoulder is made which is screwed for a purpose hereafter to be mentioned. If it is asked why I have rejected the old body for viewing opaque objects, and what is the advantage of this new one, I answer as follows. Every one accustomed to common compound microscopes may have observed a kind of milkiness in the field, so that the object appears as if seen through a kind of mist, or as if the glasses were dimmed by moisture. The greater the apertures of the little glasses are at the bottom, the more perceptible this becomes; but it can never be removed altogether, as I know from experience, by any reduction of the apertures: when an opaque object is viewed, the defect is still more striking; besides opaque objects require a great deal more light to be seen properly than transparent ones, and this kind of microscope gives very little, especially if the apertures are reduced to a proper standard. Our microscope labours under none of these defects; the image is quite clear, as if produced by a single lens, and there is abundance of light. The body of the instrument is made to *twist* into the socket of the arm C C, which is made to slide backwards and forwards in the case represented at O. The stage, D D, travels up and down with a rack and pinion, as seen in the Plate; it is five inches in length, reckoning from the brass bar, O O O; the hole E is $2\frac{1}{10}$ inches diameter; that at F one inch, and has a small notch in it. There are two holes

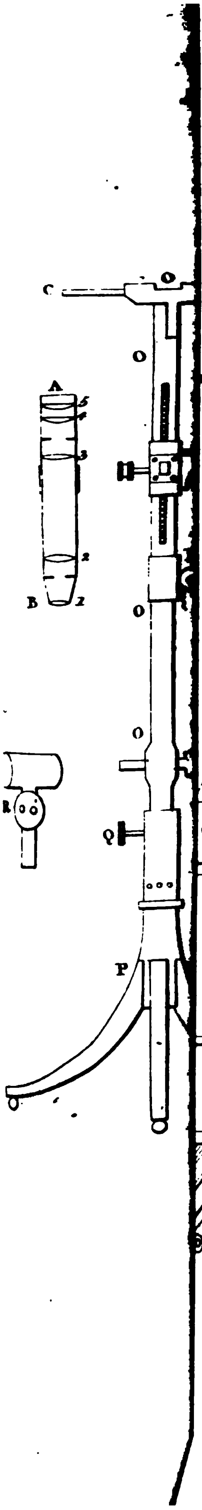
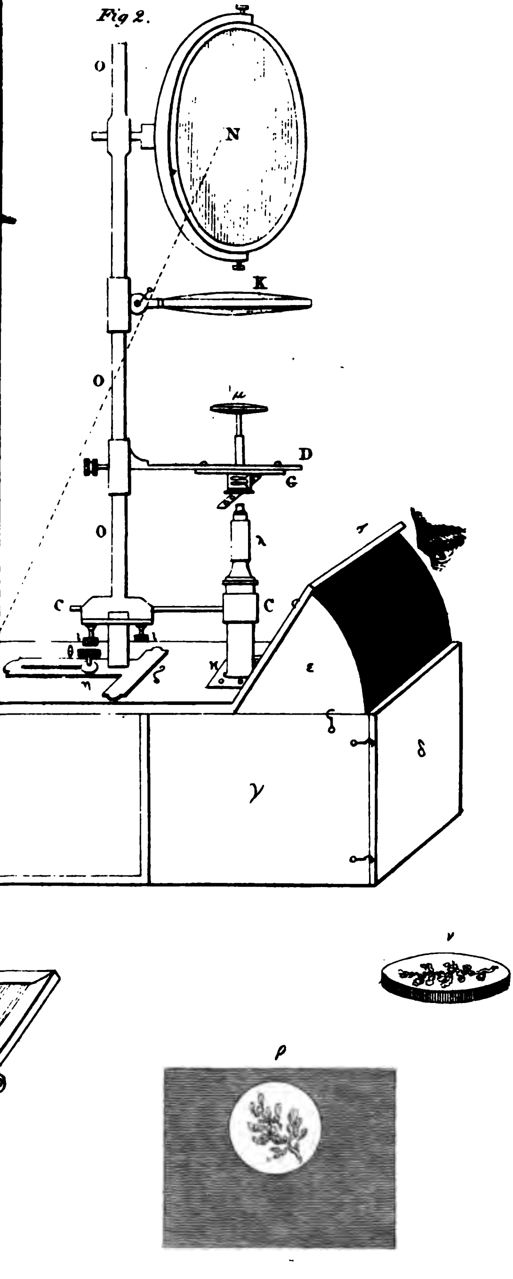


Fig 2.



in the sides of this stage, one to receive a mirror, or condensing glass, the other to receive a pair of nippers, or needle. Two little grooves are likewise made in the stage to receive the moveable stage, G, which fixes into them by means of two pins at I: this moveable stage has a hole in it, just like that at F. The mirror at S has a double motion; it is an inch and-a half diameter, including its case, plane on one side, and three inches focus on the other. When an opaque object is viewed, the body of the instrument rests over the hole F, the rays from the great mirror, N, pass up through the lens, K, through the great hole in the stage, and are reflected back upon the object by the little mirror S. When a transparent object is to be seen, the moveable stage, G, is fixed over the hole, E, and the body of the instrument is moved forward till it comes over the hole, H. The mirror is removed, and a condensing glass, μ, substituted under the stage, as seen in fig. K, is a condensing lens, 12 inches focus, and five inches diameter, including its case; it is made to move up and down the brass bar, O O O, and has a joint at L, so that it can be flapped down, when not wanted; a small hole is made through the joint, through which a pin, M, is made to pass, to fix it in an horizontal position.

N is an *oval plane mirror* whose transverse diameter is five inches, including its case; its longest diameter seven inches: it is made as light as possible, and the arm which holds it is steel; it turns round in the brass bar, O O O; the reasons why it is made *oval*, *plane*, and of the size mentioned, and why a *plane mirror* and *condensing lens* are used instead of a *concave mirror*, will be seen hereafter.

O O O is a brass bar half an inch square and $18\frac{1}{8}$ inches long; it supports the whole instrument, and is fixed into the stand, P, by means of the socket and pinching screw, Q. U is an addition to the instrument, which may be thought very nonsensical; it is something like a pair of spectacles—one side is made to screw on at A, the other is blacked; its use is to save you the trouble of keeping one of your eyes shut while you look through the instrument with the other, which is a convenience to many people: it can be used or not as is most agreeable.

When this microscope with its stand is placed on the ground, it is tall enough to reach the eye of a person sitting in a chair; and in this manner it is used.

We now proceed to fig. 2, viz. the instrument mounted as a solar microscope for viewing transparent objects.

Here the instrument we have just described will be recognized turned upside down, with some additional apparatus, N K D O C, the instrument as before.

α ϐ γ δ ε, the box which holds the instrument, when packed, two feet four inches long, $7\frac{8}{10}$ inches high, 11 inches broad; it is divided into two compartments, ϐ and γ, of which γ takes up $8\frac{1}{8}$

inches. This compartment is made pretty much like a camera obscura, except that the end δ can be removed at pleasure.

On the top of this box, a piece of brass is sunk into the wood close to the hinge of the camera, as seen at *κ*. The body of the instrument screws into this. The brass bar, *ο ο*, is attached to the box by means of the stand, ζ, made, as seen in the plate, attached by the two screws, ι ι, to the sheath which carries the arm, *c c*. This stand is again attached to the box by the screw, θ, which passes through the projecting part, η, and fixes into a piece of brass sunk into the top of the box. This projecting part has a groove in it, by means of which the instrument moves backwards or forwards, and places the body of the microscope either in its situation as in fig. 2, or in that represented at fig. 1, according as a transparent or opaque object is to be examined. In fig. 2, it is mounted for viewing a transparent body; the reader will observe the mirror, S (fig. 1), removed, and in its place the condensing lens, μ, two inches diameter, four inches focus, substituted. The moveable stage, G, is now attached, and a slider holder is fixed in it by means of the little notch. The body of the instrument is now seen much further from the brass bar than before, being placed in the axis of the mirror and lenses. The height of the stand, θ, must be made to correspond exactly with the body, so that the arm, *c c*, may just coincide with its sheath: in my instrument it is two inches high above the box; but it might be made something higher, and the brass bar, *ο ο*, might be proportionally shortened, which would make the instrument look better. The body used in fig. 2 is not the same as that in fig. 1, being precisely similar to that of a common compound microscope in its optical principle, except that the brass buttons made to screw on at its end have much smaller apertures than those generally made; likewise a piece of tube, λ, is made to slide up and down, which is used to slip over the brass button down to the slider-holder; and thereby exclude all rays from the instrument which do not come directly through the stage; this much improves the vision of many transparent objects: it can be used or not, as is most eligible.

It will no doubt appear strange that I should have rejected this kind of microscope in fig. 1, and employed it here for viewing transparent objects; but there are reasons for every thing. It is a curious fact, which I am not sufficiently skilled in optics to fathom, that the body, A B, fig. 1, though it shows opaque objects perfectly achromatic, does not seem to me to show transparent ones so, which this microscope does. Besides, I have not forced it to magnify so much as the transparent body does, because, opaque objects being seldom or never flat, only a point can be in the focus at a time, the rest being all confusion, and this in proportion to the magnifying power; so that a person does not know what he is looking at. A transparent body, being generally nearly flat, can be seen with a high power to greater advantage; but I am no advocate for microscopes of high powers. I think as much may be seen with a power of about 60 diameters (rating the standard of sight at eight inches) as can be seen with any power whatever; at least, I am sure this is the case with opaque objects. Besides, the field of view in the common microscope is larger than in mine, which, with transparent objects, is of importance, as a large portion of the object can be in the focus: with opaque ones again, as but a small portion can be seen at a time, it is of little consequence whether the field is large or not. The field and double eye-glass in the transparent microscope serve as the lowest magnifiers in the opaque one, and instead of the common astronomical eye-pieces for obtaining the higher powers, double eye-glasses might be used which would give a larger field, but it is not worth while. Though, *ceteris paribus*, the opaque microscope has much more light than the transparent one, still in the latter there is abundance of light for viewing transparent objects, which are best seen in a weak light.

I now proceed to the way of managing the solar apparatus. Fix the instrument upon its box, as in fig. 2; or if opaque objects are to be examined, mount it as in fig. 1, turned upside down. Choose a room where the sun-beams fall on the floor; place a common dressing-glass there, as σ, fig. 2. Then place the instrument upon a table, so that its side may be opposite the window, *but not where the sun-beams fall;* reflect the light from the dressing-glass, σ, to the great mirror, N, so that you may see its shadow on the wall. When you have done this, the instrument is managed just as if you were looking through it, by only reflecting the light downwards. The image appears at the bottom of the box, γ; ν is a piece of box wood 5⅜ inches diameter, covered with vellum paper; its surface is curved, so that the image may be received upon it quite perfect. ρ is a piece of pasteboard blacked, with a round hole 5⅜ inches diameter, a sheet of paper being placed under it. The end of the camera, δ, being removed, and the cloth, π, attached to the lid of the camera, τ, the image may be drawn on paper with the utmost ease. This cloth has a hole in it, as represented in the plate; it is stiffened round the edges with an iron wire, and just suits the face, so that when it is used, all foreign rays are excluded from the camera; but when two people are looking into it at once, it is not needed, as their heads exclude the light sufficiently.

There is nothing peculiar in the rest of the apparatus attending this microscope; so I shall say nothing about it, except T (fig. 1), which is a small instrument made like a little vice, but very slight, and fixes into one of the holes in the stage; it is very handy for seizing many opaque objects; for example, laying hold of the pin by which collections of insects are fixed in their boxes, and exhibiting the insect without any damage in any direction.

R is a thing which *might be made* to attach to the stand at Q, and might hold a 30 inch telescope for the eye-pieces, of which the body, A B, and the astronomical eye-pieces, would serve very well, and thereby enable a person to have a telescope at a small additional expense.

Having now, I think, thoroughly described this instrument so that the optician (for whose benefit this article is chiefly intended) may be enabled to make one without hesitation, I shall proceed to say a few words upon the nature and properties of it.

It will most probably be objected by the practical optician that this is an expensive, uncouth, top-heavy, gimcrack kind of a thing, and would not be near so saleable an article as the pretty toys and eye-traps that he is in the habit of making; that the old microscopes do very well; and that there is no occasion for putting himself out of his way with these new-fangled things.

It is not always possible to combine utility and elegance. It is necessary for this instrument to be made on a large scale on account of the size of the great mirror, which is absolutely indispensable to furnish the quantity of light required for the solar apparatus, and to enable the instrument to act by lamp light as a lucernal microscope. The mirror is made oval, that it may throw down a round spectrum through the condensing glass; and a plane mirror and lens are used instead of a concave mirror, for the same reason; otherwise the circle at the bottom of the camera will not be completely filled with light. This instrument was made by that acute and distinguished artist, Mr. Adie, of Edinburgh, inventor of the sympezometer.

Now let us consider the manifold properties and the universal application of this instrument. 1. As a compound microscope for transparent objects; 2. As a compound microscope for opaque objects; 3. As a compound solar microscope for transparent bodies; 4. As a compound solar for opaque bodies; 5. As a lucernal microscope: for all these properties are combined together in this instrument, and (under correction from better judgement), I say, are in a very reformed and improved condition.—1. As a transparent microscope. Owing to the large quantity of light furnished by the large mirror, the apertures of the object glasses can afford to be made extremely small, which greatly improves the vision, as does the tube made to slide down over them. 2. As an opaque microscope, it performs in a most superior style, owing to the clearness of the vision and the abundance of light. The small mirror, which receives all the light from the great one, is likewise a better thing than the lens usually fixed on the stage to illumine the object, as it collects all the light from the great mirror. As a transparent solar microscope, it has all the advantages which can be derived from using combined glasses instead of single lenses; for just such as a microscope is, such is the image produced by it. If it wants light, the image wants it; if distinctness or field are wanted, the image is likewise defective in these points. Another very great reformation is *cutting short the amplification of the image down to the standard of the magnifying power of the microscope producing it.* Thus if the microscope magnifies 60 times, the image is only allowed to be magnified 60 times, and so on: this is done by only suffering the rays to diverge to the distance of six inches. By these means, the maximum of sharpness and distinctness is obtained, so that the image is like a miniature picture, where every thing is seen just as if one were looking through the glasses. What purpose does it serve (save that of astonishing women and children) to suffer the rays to diverge till you have made a flea as big as a jack-ass, &c.

> Monstrum horrendum, informe, ingens, cui *lumen ademptum*—

at the expense too of all light, distinctness, and every thing valuable in vision. My instrument shows none of these wonderful wonders. I hold them in supreme contempt, but in *real magnifying power* it is nevertheless greatly superior.* No sensible optician ever forces a telescope, or microscope, to a higher power than it is capable of bearing, as the object is not only seen no better (though larger), but a vast deal worse. If it is asked why I did not, at least, suffer the rays to diverge to eight inches instead of six, eight inches being the standard of sight, I answer, I do not believe eight inches to be the standard of sight, but that it is much less than that. I am not conscious that I am either long or short sighted. I can see to read moderately sized print at the distance of five feet, and I can read the same 4½ inches from my eye. When I go to look narrowly into any thing, I generally look at it 4½ inches distance. Let any lady, with acknowledged good eyes, take a microscopical object, such as a transparent slider, or some such thing, and let her mark the distance she places it from her eye, when she sees it to the best advantage, so as to see most into its nature. I am sure 99 out of 100 will place it nearer than eight inches. However, I have assumed six inches.

As an opaque solar microscope, this instrument possesses the same advantages over the common one as the transparent part does; it does not, however, magnify above 60 diameters (common computation), and would, in my opinion, be of no use if it did. I may mention that the common transparent body will not form an image of an opaque body, from its affording so little light.

As a lucernal microscope, this is likewise superior to the common one, for which all that is necessary is to place a fountain lamp on the floor, on one side of the instrument, on the same level with the great mirror, and about two feet distant from it, so that the light may meet the long axis of the ellipse at a right angle; then every thing goes on as before; and I think the transparent part of the instrument especially is never seen to more advantage than in this way. The vision altogether is very superior to that of the common lucernal, which is always full of colour, very indistinct, and distorted at the edges of the field. Our instrument will not, however, produce any image in the camera by lamp light—at least, it is a mere shadow. I have tried every possible kind of microscope, simple and compound, to endeavour to get a decent image by lamp light, but have been totally unsuccessful; the reason of which seems to be this: If you allow the apertures of the lenses sufficient diameter to give a requisite quantity of light, the image is quite confused, and full of colour; if you reduce them to the proper standard, the lamp will not afford light to show it, and here the matter rests. *Incidit in Scyllam*, &c. The utmost which can be done is to produce an image of a transparent object (an opaque one is out of the question), of which you are enabled to see the outline and something of the colour very slightly magnified; but into the texture and minutiæ of which, you can see nothing. Of this description is the image of a common lucernal; and, in my opinion, it is not worth looking at. The only remedy for this which occurs to me, would be a lamp which should give as much light *close to the instrument* as the sun does at his natural distance.

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* The real power of the glasses producing the image is about four times greater than that of the lenses generally used for solar microscopes.

The best way to procure an image by lamp light in our microscope is to take the *opaque body* at its lowest power to view a *transparent object* (as this body gives much more light than the other), then to get a high stool and place it upon the table with the lamp opposite the large mirror, and proceed as with the sun. The camera must be quite dark. An image will be formed so that any body may affirm the instrument produces an image by candle light; but this is all that can be said of it.

It would not be amiss in packing the microscope to keep the opaque apparatus distinct from the transparent, so that a person inexperienced in microscopes might more readily learn to manage it: from the variety of purposes to which it is subservient, it is somewhat more complicated in its construction than microscopes usually are.

I have neglected to describe a kind of slider which I use in my microscope; it is composed of a glass tube, flattened, and drawn out to the size of a common slider, and polished on one side: its use is to hold microscopical objects which will not keep in a dry state, such as pieces of finely injected membrane, petals of flowers, and the like; these little preparations are introduced into the slider, which is then filled with spirits, and covered at the end with a bit of bladder secured by a wax thread.

I now proceed to fig. 3, which is a compound microscope, which would be very useful for dissecting insects, as it shows the image erect like a single lens; its power is about 60 (common computation), nor will it magnify above that with distinctness; it is composed of seven lenses, and, nevertheless, shows the object very clear and distinct; its body is $10\frac{1}{8}$ inches long; it is in its principle the eye-piece of a small telescope, connected with the field and double eye-glass of the common compound microscope; the little eye-piece at the bottom is $3\frac{6}{8}$ inches long; as I have it, a body of this nature is made to screw into the arm of the instrument like the other; but in this state it is not sufficiently steady to be used with comfort, as a very slight tremor is perceptible when you are working. I recommend a stand for it, such as that in fig. 3, made very solid, with a rack and pinion to move the body like that in Culpepper's microscope.

The advantages of this instrument would be that, being to be used for a continuance, it would not strain the eye like a single lens of the same magnifying power, which would need to be $\frac{1}{8}$th of an inch focus; that there would be abundance of room to manage the dissecting instruments, as the focal distance of this microscope from the object is half an inch; and that the operator would not be under the necessity of putting his nose close down over the object, and thereby darkening it, so that the light would require to be thrown up from below and reflected back upon the object by a silver cup.

I do not see any particular utility in this last instrument, except as a dissecting microscope. I have now given a plain account of this instrument without any reference to theory, or any display of algebra and mathematics. I have written for the practical man only, to whom I recommend the instrument as a valuable article of his trade, the cost of which will not exceed that of a good compound microscope of the common make, with a transparent and opaque solar apparatus, and will, I think, give much more satisfaction; at least, to those who can distinguish a bright, clear, achromatic, distinct image, from a distorted, dull, confused one, and who prefer in a solar microscope an image abundantly magnified, and as sharp as a miniature picture, to a huge, indistinct shadow. It is the established practice of every inventor to extol the merit of his own production, and to decry all others; but I do not think I have asserted any thing here of mine which will not bear the closest examination by those most skilled in optical instruments.

Article X.

Notice of some Animals from the Arctic Regions.
By Dr. Leach.

(To Dr. Thomson.)

MY DEAR SIR,

In compliance with your wishes, I now transmit to you a hasty list of the mammalia and birds that have been received from the Northern Expeditions, and which have since been sent to the British Museum by the Admiralty.

I remain, yours faithfully,
W. E. Leach.

MAMMALIA.

1. *Ursus Albus*, Brisson, Jonston (White, or Polar Bear).—A very large specimen, nearly nine feet in length, was brought home by Capt. Ross. It was skinned and prepared by Mr. Beverly, who devoted much time and attention to its preservation.

2. *Canis*.—A variety approaching to the wolf in many points of external character and in voice. It wants the thumb on the hinder feet.

Baffin's Bay, Capt. Ross.

3. *Vulpes Lagopus* (Arctic Fox).—This animal was received alive, and did not emit the disagreeable odour of the common fox in a great degree: this has been observed before. Coast of Spitzbergen, Capt. Buchan.

4. *Phoca Fœtida?* Müller, Young (Jacob's bite), June 30, Capt. Ross.

5. *Trichechus Rosmarus* (Walrus).—The head only was received, from Capt. Ross.

6. *Lepus* ——?—Certainly distinct from our White Hare (*Lepus albus*, Brisson), which again seems to be distinct from the *variabilis* of Pallas.

It is of the size of the common hare, and of a white colour. The back and top of the head are sprinkled with blackish-brown (nigricante-fusco) hair, banded with white; the sides of the neck are covered with hair of the same colour interspersed with white. The extreme tips of the ears are tipped with black, intermixed with white. The insides of the ears have a few black hairs mingled with the white. As the skeleton was not brought home, it will be impossible to clear up much respecting the three white-coloured hares above-mentioned. It was killed on Sept. 1, in lat. 73°, on the west side of Baffin's Bay.

7. *Cervus Tarandus* (Rein Deer). Coast of Spitzbergen, Capt. Buchan.—The heads only of this animal were received. The horns in the growing state are covered with woolly down, much longer in proportion than that on those of the various deer that are domesticated in this country.

AVES.

1. *Falco Smirillus* (Merlin Falcon). Lat. 65°, Capt. Ross.

2. *Vitiflora Ænante* (White-rumped Wheatear).—Killed at sea, by W. E. Parry, Esq.; lat. 59° 51′ N.; long. 11° 21′ W May 6.

3. *Emberiza Nivalis* (Snow Bunting).—Capt. Ross.

4. *Hæmatopus Ostralegus* (Common Oyster-catcher). Ferroe. F. Franks, Esq.

5. *Pelidna Alpina* (Common Dunlin).

6. *Tringa Islandica.*

7. *Lobipes Hyperboreus,* Cuvier (Red Lobefoot), commonly placed in the genus *Phalaropus.*

8. *Rallus Sericeus* (Common Rail).

9. *Uria Francsii* (Franks's Guilemot).—This is a new species of which I have given a description to the Linnean Society. It was first killed off Ferroe, by F. Franks, Esq. who sent it to me; it has since been received from all the ships employed in the northern expedition.

10. *Grylle Scapularis* (White-winged Scraber).—All the ships met with this bird. It is commonly denominated *Black Guilemot,* but has been referred to a distinct genus, named *Cephus* by Cuvier; a name which I cannot, for many reasons, adopt.

11. *Mergulus Malanoleucos* (Common Sea-Dove).—Killed by all the ships.

12. *Fratercula Glacialis* (Northern Puffin).—This new species, on which I have sent a paper to the Linnean Society, was killed off the coast of Spitzbergen

13. *Procellaria Glacialis* (Fulmar Petrel).—Spitzbergen and Baffin's Bay.

14. *Larus Eburneus* (Ivory Gull).—Baffin's Bay.

15. *Larus Rissa* (Kittiwake Gull).—Spitzbergen.

16. *Larus Canus* (Common Gull.)—Ferroe. F. Franks, Esq.

17. *Larus* —— ?—A large species not yet determined. Baffin's Bay.

18. *Larus* —— ?—Young, of a large species not determined.

19. ———— ? Sabini.—A paper on this bird (which forms an intermediate genus between *Larus* and *Sterna*) has been read to the Linnean Society, by Joseph Sabine, Esq. who named it *Larus Sabini,* after his brother who first killed it.*

20. *Sterna Hirundo* (Common Tern).—Ferroe and Spitzbergen.

21. *Stercorarius Cepphus* (Arctic Jager).—Baffin's Bay.

22. *Catarracta Fusca* (Squa Catarractes).—Ferroe. F. Franks, Esq.

23. *Somateria Mollissima* (Cuthbert's Eider).—Baffin's Bay, Spitzbergen.

A great number of other species were killed by individuals, which have not been deposited in the British Museum.

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* See Linnean Society report, p. 67.

ARTICLE XI.

ANALYSES OF BOOKS.

Memoirs of the Wernerian Natural History Society. Vol. II. Part II. For the Years 1814, 1815, 1816.

THIS part contains the following papers:

I. *On the Greenland, or Polar Ice.* By W. Scoresby, Jun. Esq. M.W.S.—Mr. Scoresby has been in the habit of going annually to the Greenland seas, for these many years past, as a whale fisher. Being a man of excellent abilities, of good education, and a zealous observer, he has collected a vast number of curious and important facts, which must, when they are given to the public, contribute materially to the improvement of meteorology; for the weather in the polar regions must influence materially the winds and currents of the Atlantic and Pacific Oceans; which, in their turn, exercise a material influence upon the continents which lie on either side of them. In our report of the proceedings of the Wernerian Society, vol. vi. p. 142, &c. we gave an account of the present paper; but as the subject is very curious in itself, and particularly interesting at the present moment, when the public attention is drawn to the two voyages of discovery lately made to the arctic regions, we are induced to give an analysis of it, even at the risk of repetition.

The whalers have distinguished the polar ice by a variety of names according to its state. A large ice plain, extending further than the eye can reach, is called a *field*. When a field, in consequence of a heavy swell, is broken into pieces, not exceeding 40 or 50 yards in diameter, which remain in close contact, so that they cannot be seen over from the ship's mast, they are termed a *pack*. When the collection of pieces can be seen over, and when it assumes a circular, or polygonal form, it is called a *patch*. When it is long and narrow, it is called a *stream*. Pieces of very large dimensions, but smaller than fields, are called *floes*. Small pieces which break off and are separated from the larger masses by the effect of attrition, are called *brash ice*. Ice is said to be *loose*, or *open*, when small pieces are so far separated as to allow a ship to sail freely among them. This has likewise been called *drift ice*. A *hummock* is a protuberance raised upon any plane of ice above the common level; it often attains the height of 30 feet, or upwards. A *calf* is a portion of ice depressed by the same means as a hummock is elevated. Any part of the upper surface of a piece of ice, which comes to be immersed beneath the surface of the water, is called a *tongue*. A *bight* is a bay, or sinuosity, on the border of any large mass or body of ice.

When the ice is porous, white, nearly opaque, but having a greenish shade of colour, it is considered by the whalers as formed by the congelation of sea-water, and called *salt water ice*. When this ice is thawed, it sometimes yields fresh water, and sometimes brackish water. The specific gravity of this ice, according to Mr. Scoresby, is 0·873.

The name *fresh water ice* is applied to ice which has a black appearance while floating in the sea, but a beautiful green hue and transparency when removed into the air. Its transparency is usually interrupted by numerous small, pear-shaped air-bubbles. When formed into convex lenses, it collects the sun's rays into a focus, and sets fire to gunpowder, &c. precisely as a glass lens would do. Its specific gravity, according to Mr. Scoresby, is 0·937.

It has been conceived by many, that the ice which covers the polar seas has its origin from the land; but Mr. Scoresby is of opinion that the vicinity of land is not necessary for the formation of ice. He has seen the sea freeze at a distance from land, both when smooth and when agitated by the wind; and he describes the appearances which take place in both cases. He conceives that, during the summer months, the polar ice splits, and one portion separates to a distance from the other. In winter, the interval between these two portions freezes, and becomes covered with snow. This snow is melted during the ensuing summer, and the pond of water, thus formed upon the ice, freezing the ensuing winter, constitutes a field of ice.

Fields have a constant tendency to drift to the south-west. This occasions the destruction of many, whose place is supplied by others from the pole. Fields sometimes acquire a circular motion of three or four miles an hour. When two fields moving different ways meet, they act upon each other with prodigious energy, breaking each other in pieces, and piling up the fragments to a great height. When ships are interposed between two fields, in such a case, the consequence is alarming, and often destructive.

The term *iceberg* is commonly applied to those immense bodies of ice situated on the land, filling the valleys between high mountains, and generally exhibiting a square perpendicular front towards them. They recede backwards inland to an extent never explored. Large pieces may be separated from these icebergs in the summer season. These masses, floating in the sea, still retain the name of *icebergs*, *ice islands*, or *ice mountains*. In height, above the surface of the sea, they may be 100 feet, or more, and below the surface, 100 yards, or more; while their diameter varies from a few yards to some miles. They abound in Davis's Strait; but are few in number and small in size off the coast of Spitzbergen. On that account, Mr. Scoresby thinks that they rather originate in sheltered bays of the land than from land icebergs. They occur also at some hundred miles' distance from land towards the north. The perpetual accumulation of snow, &c. from the atmosphere during a long succession of centuries, is sufficient, in the author's opinion, to account for the existence of the largest ice mountains than can be supposed to exist.

The icy barrier at the return of spring exhibits the following general outline. After doubling the southern promontory of Greenland, it advances in a north-eastern direction along the east coast enveloping Iceland as it proceeds, until it reaches John Mayne's Island, in latitude 71° N., longitude about 5½° W. Passing this island on the north-west, but frequently enclosing it likewise, it then trends a little more to the eastward, and intersects the meridian of London in the 71st or 72d degree of latitude. Having reached the longitude of 6, 8, or perhaps 10 degrees east in the 73d or 74th degree of north latitude, it suddenly stretches to the north, sometimes proceeding on a meridian to the latitude of 80°; at others, forming a deep sinuosity, extending two or three degrees to the northward, and then south-easterly to Cherry Island, which having passed, it assumes a direct course a little south of east, until it forms a junction with the Siberian, or Nova Zemblan coast. When the ice at the extremity of this remarkable bay occurs so strong and so compact as to prevent the approach to the shores of Spitzbergen, and the advance northward beyond the latitude of 75°, or 76°, it is said to be a *close season*. On the contrary, it is called an open season when an uninterrupted navigation extends along the western coast of Spitzbergen to Hackluyt's Headland. It is about latitude 80° that the haunt of the whale occurs. The great object of the whaler is to get into that situation, and much dexterity and intrepidity are necessary to enable him to get as speedily as possible into the proper fishing latitude. The loose ice which opposes his passage northwards has disappeared by the middle of June, when he has to return home.

II. *On the Mineralogy of the Read Head, in Angusshire.* By the Rev. John Fleming, D.D. F.R.S.E.—This district, which may be considered as the termination of the great valley of Strathmore, consists partly of alluvial beds and partly of floetz rocks. The alluvial beds consist of sand and gravel, and may be seen along the banks of the Brothick and the Lunan, two small rivers which run into the sea at this place. The beds of sand are parallel to each other; but they dip in some places at an angle of 24°. Dr. Fleming adduces this fact as a demonstration that the Huttonian axiom, *that beds deposited at the bottom of a liquid must be horizontal*, is not always true.

The floetz rocks in this district are the *old red sandstone*, which skirts the Grampians on both sides, and runs from the east to the west sea. Dr. Fleming considers the hills of Kinnoul and Moncrief, the Ochil Hills, and Arthur's Seat, as belonging to the old red sandstone, and constituting beds in it. If this opinion, which was advanced by Professor Jameson in a paper published some years ago in the *Annals of Philosophy*, be correct, it will follow from it that most of the rocks supposed hitherto to be peculiar to the newest floetz trap formation, belong to the old red sandstone, and constitute subordinate beds in it. Thus if East Lothian consists of old red sandstone, the porphyry slate of North Berwick law, and Traprene law, and the floetz trap rocks of Dunbar, must constitute beds in the sandstone. This would probably be made out in a satisfactory manner by travelling along the south coast of the Frith of Forth from Prestonpans to Dunbar, as the rocks are exposed for the greatest part of that way.

III. *Description and Analysis of a Specimen of Native Iron found at Leadhills*. By Mr. H. M. Dacosta, M.W.S.—The specimen was found associated with galena, and was discovered by the workmen from its resisting the blows of a hammer. It possessed the external characters of iron, and was found composed of

Iron	16·5
Silica	1·0
Loss, chiefly sulphur	0·5
	18·0

IV. *Mineralogical Observations in Galloway*. By Dr. Grierson.—There are three different granite districts in Galloway. Dr. Grierson formerly gave an account of the middle, or Dee district, in a paper published in a former volume of the *Annals of Philosophy*. The object of this paper is to give an account of the western, or Doon granite district. This district lies between Loch Doon and Loch Dee, and probably extends eight miles in length and four miles in breadth. It is covered on all sides by a rock, to which the author has given the name of *compact gneiss*. This gneiss rock can be traced sometimes for a mile, and sometimes only for a few hundred yards. Greywacke always covers it; at least, Dr. Grierson no where found the greywacke in contact with the granite. Fragments of the gneiss are frequently met with in the granite. It contains likewise numerous beds of felspar porphyry.

V. *Lithological Observations on the Vicinity of Loch Lomond*. By Dr. Macknight.—The rocks round Loch Lomond are mica slate, which continues to Ben Lomond, which is itself composed of it. The mica slate contains thick beds of felspar porphyry and greenstone. Immediately to the south of Ben Lomond, the clay slate rocks commence. At Luss and Camstradden, they are quarried for roofing slate. The clay slate is followed by greywacke and greywacke slate, and these transition rocks are followed by the old red sandstone.

I had an opportunity last autumn of examining a small portion of the east bank of Loch Lomond, near Buchanan House, just where the greywacke terminates. The next rock is a limestone, which is probably transition, though it does not possess the usual characters of that kind of rock. The limestone is succeeded by a very coarse gravel stone, composed almost entirely of rounded quartz pebbles, seemingly cemented by a quartzy matter. This rock is obviously a modification of the old red sandstone, which a little to the south appears in its usual characters. This part of the Grampian agrees in its structure with every other cross section of these mountains which I have had an opportunity of examining.

VI. *Description of Ravensheugh.* By Dr. Macknight.—This is the name given to a point of the coast included in the Earl of Haddington's pleasure-grounds at Tyninghain, about six miles north-west of Dunbar. It consists of a set of beds forming a ock, which exposes a precipitous front to the sea, about 40 or 50 feet in height. This rock is composed of floetz trap beds reposing on old red sandstone. The trap beds consist of basalt, red and green trap tuff, impregnated with lime, clinkstone, and porphyry slate. The curious circumstance attending it is, that the beds of sandstone over which this floetz trap rock lies, seems to run beneath the basalt in every direction, assuming the form of a vast cup, or cavity, filled with the floetz trap. This depression Dr. Macknight accounts for, by supposing that the floetz trap was deposited upon the sandstone before this last rock was completely deposited. Hence it would, he thinks, squeeze down the sandstone, and cause the depression which exists.

If this explanation be well founded, the specific gravity of the sandstone below the floetz trap would be greater than at a distance from it.

VII. *Hints regarding the Coincidence which takes place in the Pressure of the Atmosphere at different Latitudes, and at nearly the same Time.* By the Right Hon. Lord Gray, F.R.S. Lond. and Edin. &c.—His Lordship shows, by a set of curves, exhibiting the march of the barometer, during two years, at Gordon Castle, Kinfauns Castle, Greenwich Observatory, and Plymouth, that the rise and fall was nearly simultaneous at all these places. He thinks that this will hold nearly from the pole to the equator, and is exceedingly anxious to have the means of verifying his conjecture by observations made in the southern hemisphere.

(*To be continued.*)

Article XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Dec. 10.—A paper, by M. Theodore de Saussure, was commenced, entitled Observations on the Decomposition of Starch by the Action of Air and Water at common Temperatures.

Dec. 17.—The above paper was concluded. A portion of starch simply boiled in water was exposed for two years under a glass jar in a temperature between 68° and 77°. At the end of this time, about $\frac{1}{3}$d of it was found converted into saccharine matter, having all the properties of sugar prepared from starch by the action of sulphuric acid, according to the method of M. Kirchhoff. On observing this curious circumstance, the author was induced to examine more attentively the nature of the changes which took place. He found that, besides sugar, a species of gum was formed, similar to that obtained by roasting starch; also a peculiar intermediate substance, which he denominated *amidine*, while a substance remained, insoluble in water and acids, which gave a blue colour with iodine, and was probably starch somewhat altered in its properties. The author states, that when air is present during the above process, water and carbonic acid gas are given off in considerable quantities, and that charcoal is deposited; but on the contrary, that when air is excluded no water is formed, that only a little carbonic acid and hydrogen are extricated; and that no carbon is deposited. The author was unable to determine whether the presence or absence of air affected the quantity of sugar obtained. The paper was concluded with some remarks, which rendered it probable that water is fixed, during chemical operations, upon organized substances more frequently than is usually supposed.

At this meeting also, a paper, by C. Babbage, Esq. was read, on the solution of some problems relating to the games of chance. The object of the author was to show, that a certain series of questions, hitherto supposed to lie beyond the reach of analytical investigation, might be adapted to algebraic reasoning.

Dec. 24.—A paper, by Capt. Duff, R. N. was read, on the prevention of the dry rot in timber, by means of peat moss. The author, after stating the well-known effects of peat moss in preserving wood for ages unaltered, suggests that a set of experiments should be made to ascertain the effects of impregnating timber, both sound and already partially decayed by the dry rot, with the water from peat mosses, with the view of determining whether it possesses any power in preventing, or suspending, the insidious operation of that destructive agent.

LINNÆAN SOCIETY.

Nov. 3.—A paper, by Dr. Leach, was read, on the Cymothoada, a family of Crustacea, with Sessile eyes.

Nov. 17.—The Society met; but adjourned immediately on account of the death of the Queen.

Dec. 15.—A paper, by Joseph Sabine, Esq. F.R.S. and F.L.S. was read, containing an account and description of a new species of Gull (Larus Sabini), lately discovered on the west coast of Greenland, and which is characterized by having a furcate tail, like the Tern.

At this meeting also, part of a paper, by Joseph Smith, Esq. F.L.S. was read, entitled "Some Account of the Botany of Jersey, Guernsey, Alderney, and Sark.

ARTICLE XIII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Action of Iron on Water.*

M. Guibourt has shown by a set of experiments, which appear accurate, that iron has the property of decomposing water at the common temperature of the atmosphere. The decomposition is most rapid, when the quantity of iron bears a great proportion to the quantity of water. In that case, the temperature rises considerably, the decomposition goes on more rapidly in proportion as the temperature is more and more elevated.—(Journ. de Pharm. June, 1818, p. 241.)

M. Robiquet has ascertained that the black oxide of iron formed by the action of water on iron at the ordinary temperature of the atmosphere, is exactly similar to the oxide formed by the action of red hot iron on steam. Now it is well known that this last oxide is a compound of one atom of protoxide and one atom of peroxide. The octahedral iron ore of mineralogists is a similar compound.—(Ibid. p. 308.)

II. *Carbonate of Iron.*

As far as we know at present, the only oxide of iron capable of combining with carbonic acid is the protoxide. Carbonate of iron found native is a compound of an atom of carbonic acid and an atom of protoxide of iron. I have never been able to succeed in my attempts to form a percarbonate of iron, though analogy leads me to suspect the possibility of the existence of such a salt.

III. *Action of Prussian Blue on Starch.*

M. Vincent, an apothecary in France, has published the following curious fact. If four parts of starch and one part of prussian-blue be mixed and triturated together in a mortar, so as to make as intimate a mixture as possible, and this mixture be boiled in a considerable quantity of water, the liquor, before it reaches the boiling temperature, acquires a green colour: it then becomes brown, and there remains a precipitate, which does not recover its blue colour, though treated with acids. The liquor has the property of forming a very fine prussian blue, when treated with a solution of sulphate of iron mixed with an equal volume of solution of chlorine. When the liquid is evaporated, no gluey substance is deposited; but if it be reduced to a small volume, and allowed to cool, it gives a glutinous matter, which dries in the open air, and is again easily dissolved in water. The starch then is altered in its nature, and converted into a kind of gum.—(Ibid. p. 325.)

IV. *Deaths in Paris during* 1817.

The following tables are so curious and so instructive that I have copied them from the annual report published in the Journal de Pharmacie.

Deaths in 1817	21,386
——— 1816	19,805
Excess in 1817	1,581

These deaths consist of 13,555 who died in their own houses; viz.:

Males	6,599	13,555
Females	6,956	

The remainder consist of 276 dead bodies deposited in the *Morgue*, and 7,827 who died in the hospitals, viz.

Males	3,898	7,827
Females	3,929	

The number of persons who died of the small-pox in 1817 was 488, viz.

Males	250	486
Females	236	
The number in 1816 was		150
Excess in 1817		336

The 276 dead bodies deposited at the *Morgue* in 1817 consisted of

Males	205	276
Females	71	

The number of drowned in 1816 was	278
And that of suicides	188
Suicides in 1817	197

If we admit that at least one half of the drowned persons underwent a voluntary death, the number of suicides in 1817 will amount to 335, or to more than six every week.

In 1808, 1809, 1810, the annual number of suicides was from 50 to 55. This number has increased progressively since 1812.

V. *Saffron supposed to prevent Sea Sickness.*

M. Cadet, who spent part of the summer of 1817 in London, mentions that when he crossed the channel from Calais to Dover, he observed an English gentleman with a bag of saffron suspended over his stomach. On inquiring the reason, he was told by the gentleman that it was a practice which he always followed when crossing the channel, because it preserved him from sea sickness. The remedy was found out, he said, in the following way. A small merchant, who had occasion to make frequent voyages, was always tormented with sea sickness when on ship-board. One day he embarked, after purchasing a pound of saffron, which he put under his shirt in order to avoid paying duty for it. He escaped without experiencing any sea sickness, though the sea was rough. Ascribing this lucky escape to the saffron, he communicated his discovery to several of his friends, who made repeated trials of the remedy, and always with success.

I have translated the above passage from the Journ. de Pharm. July, 1817, p. 335, though far from implicitly believing that saffron is likely to cure this hitherto incurable malady; but that the alleged cure may be generally known, and that its efficacy may be tried by those who have occasion for the remedy.

VI. *Purification of Platinum.*

The Marquis of Ridolfi has proposed a method of purifying platinum, which seems worth the attention of those who have occasion for platinum vessels for the purposes of manufacture, as it would materially diminish the price of that expensive metal. It is obvious that the platinum will not be obtained quite free from lead; but it is not probable that the small portion of that metal still left in it would render it injurious to the sulphuric acid makers, who are the manufacturers that chiefly employ platinum upon a great scale.

Ridolfi separates mechanically such foreign bodies as can be detected by the eye in crude platinum. He then washes it in dilute muriatic acid. The next step of the process is to fuse the crude metal with four times its weight of lead, and to throw the melted alloy into cold water. It is then pulverized, mixed with its own weight of sulphur, and thrown into a hessian crucible previously heated to whiteness. A cover is placed on the crucible, and it is kept at a red heat for 10 minutes. When allowed to cool, a brilliant metallic button is found under the scoriæ, composed of platinum, lead, and sulphur. A little more lead is added, and the alloy is fused a second time. The sulphur separates with the new scoriæ, and there remains an alloy of platinum and lead. This alloy is heated to whiteness, and while in this state, hammered upon an anvil with a hot hammer. The lead is squeezed out, and the platinum remains.

Platinum obtained in this way is as malleable and ductile as the finest platinum. Its specific gravity is said to be 22·630. If so, it must be alloyed with lead; for pure platinum is not so heavy.

VII. *Reumic Acid.*

Some years ago a paper by Mr. Henderson, on the acid of rhubarb, was inserted in the *Annals of Philosophy*. The result of his experiments led him to consider it as a peculiar acid, which he distinguished by the name of reumic acid. The only characteristic property, however, by which he was able to distinguish it, was that of dissolving mercury.

A set of experiments on the juice of the *rheum* ponticum has been lately made by M. Lassaigne, with a view of verifying the results obtained by Mr. Henderson. The juice of this plant is abundant, and very acid; but the acid possesses all the characters of the oxalic, and has no action whatever upon metallic mercury. The *reumic* acid, of course, does not exist as a peculiar acid.—(See Ann. de Chim. et Phys. viii. 402.)

VIII. *Perchloric Acid.*

Sir Humphry Davy has verified the curious discovery made some years ago by Count von Stadion, of a combination of chlorine and oxygen, containing more oxygen than chloric acid, and which, therefore, may be distinguished by the name of perchloric acid. A particular account of the experiments of Count von Stadion will be found in the *Annals of Philosophy*, ix. 22. I have likewise given an account of this curious acid in the last edition of my System of Chemistry.

IX. *Aurora Borealis at Sunderland.* By Mr. Renney.

(To Dr. Thomson.)

SIR, *Bishopwearmouth, Nov.* 4, 1818.

On Saturday night, the 31st ult. between seven and eight o'clock in the evening, was observed, at Sunderland, that beautiful phenomenon the aurora borealis, in a more singular form than I have at any time before seen it. Due north appeared a very dark dense cloud, nearly in the form of a segment of a circle; the altitude about 15°, from behind which issued upwards equally fine radii, about 20° in length, and gave light equal to the twilight in summer, casting a sensible shadow against a wall, facing the north, and had a very fine appearance. The remainder of the hemisphere was perfectly clear. About 11, the cloud had the same appearance, but the radii very much altered; in some places, hardly to be perceived; in others, very strong; and many of the radiant points extended southward of the polar star, and very brilliant. An hour afterwards, the radiant points were less vivid, and the dark cloud seemed to break off towards the south. Perhaps, it may be worthy of remark, that a south wind generally prevails shortly after the appearance of this phenomenon.

Many persons in our streets seemed to consider this phenomenon as intended by the Supreme Disposer of events, to foreshow some heavy calamity coming upon the earth. But we are not supported by just principles of reason in forming such a conclusion; for let it be considered, that at Greenland it is seen almost every night, and was very useful to three of our countrymen who wintered there, being left at Spitzbergen in Aug. 1630, till the following year, and must be so in general to the inhabitants of that dreary region. Very frequently it is seen at Iceland, Lapland, and Siberia, and about the Shetland Isles, where the inhabitants know this phenomenon by the appellation of the merry dancers; and how are we to ascertain to what state, or nation, such calamity is portended by this phenomenon, or when it will happen? Are those nations where it is seen so constantly to be as constantly visited? and are they always visited when this sign appears? The fact is quite otherwise; for at such times as this phenomenon has been most extraordinary, so as to merit the regard of historians, nothing peculiarly tragical is related in connexion with it, or, at least, historians have not noticed any such calamity, or could not find any such to apply to it; therefore, we should regard the aurora borealis not as a token of Divine displeasure, but what it really is, one of the ordinary phenomena of nature, to be ranked with comets, meteors, mock-suns, &c. Should you think the above interesting to the readers of your journal, the insertion will much oblige,

Sir, your obedient and humble servant,

ROBERT RENNEY.

Erratum.

Vol. ix. p. 251, line 4, and index, *for* Pensey *read* Renney.

X. *Death of Professor Bucholz.*

The chemical readers of the *Annals of Philosophy* will learn with regret the death of Christian Frederick Bucholz, an Apothecary, Doctor of Sciences, and Professor of Chemistry at Erfort, in Saxony. He died on June 8, 1818. In the Journ. de Pharm. (Oct. 1818, p. 487), where Bucholz's death is announced, he is said to have been in the 49th year of his age. But I conceive that there must be some mistake in this statement; for the first chemical paper of Bucholz, on the mode of preparing the fusible salt of urine, was published in 1771, or 47 years ago (Chym. Abhandlung vom schmelzbare Urinsalze. In N. Hamb. Magazin, p. 58). Now we cannot suppose him to have begun to publish the results of his chemical experiments till he was at least 15 or

16 years of age. I conceive, therefore, that he must have reached at least the age of 60. His health was for many years excellent; but it was injured during the last war of Bonaparte in Germany, particularly by the siege of Erfort. His sight became very feeble during the latter years of his life: he became almost blind, which threw him into a profound melancholy. His character is represented as very amiable. He has left behind him a widow and one son, who is said to possess the abilities of the father.

Bucholz was one of the most active and accurate chemists which Germany possessed. His publications are exceedingly numerous, and all of them stamped by the most patient industry. He was an apothecary, and devoted much of his time to the improvement of his art. He was in the habit of publishing an annual volume on the subject. He published three volumes of chemical experiments, under the title of "Beitrage." And a vast number of chemical papers by him are to be found in Crell's Annals, Scherer's Journal, Gehlen's Journal, Trommsdorf's Journal, and Schweigger's Journal.

XI. *New Yellow Dye.*

A chemist in Copenhagen is said to have discovered a new brilliant yellow dye, which possesses a great deal of permanence. He cuts off the top of the common potatoe plant while in blossom, and bruises it in order to extract the juice. Cotton, or woollen cloth, steeped in this juice for 48 hours, acquires a fine, solid, durable, yellow colour. If the cloth be now put into the blue vat, a very fine green colour is obtained, which is not liable to fade. See the Journal of Toulouse, called "Ami du Roi," No. 82.

XII. *New Observations on the Planet Uranus.*

When Herschel ascertained in 1781 the motion of Uranus, astronomers endeavoured to ascertain whether this planet had been already observed as a fixed star. M. Bode discovered two observations of the planet, the one in the catalogue of Flamsteed, and the other in that of Tobias Mayer. Lemonnier, on his part, ascertained that he had himself observed it three times. More lately, Messrs. Bessel and Burckhardt have found several positions of the new planet in the catalogues of Flamsteed and Bradley. In order to make the tables, which he is just going to publish as perfect as possible, M. Bouvard has had the patience to go over line by line the manuscript registers of Lemonnier, and has discovered that this astronomer had observed Uranus 12 times between Oct. 14, 1750, and Dec. 18, 1771. The disorder of these registers, which rendered the labour of M. Bouvard very disagreeable, can alone explain how Lemonnier had not perceived that the star which he observed had a motion of its own. The following is the result of the 12 observations of that astronomer.

Mean time reckoned from midnight.				Appar. right ascension.			Declination.			
1750.—Oct. 14, at	19h	5′	19″	324°	30′	28·2	15°	1′	42·0″	S.
—— Dec. 3,	16	50	16	324	34	53·5	14	53	19·0	S.
1764.—Jan. 15,	17	12	23	12	37	30·0	4	48	47·0	N.
1768—Dec. 27,	19	38	45	31	26	52·0	12	15	35·0	N.
—— Dec. 30,	19	26	49	31	24	45·8	12	14	55·0	N.
1769.—Jan. 15,	18	29	0	31	22	7·7	12	14	56·0	N.
—— Jan. 16,	18	25	6	31	12	23·4	12	14	36·3	N.
—— Jan. 20,	18	4	11	31	24	6·6	12	15	10·0	N.
—— Jan. 21,	18	0	18	31	24	23·8	12	15	31·8	N.
—— Jan. 22,	17	56	23	31	25	4·7	12	15	45·7	N.
—— Jan. 23,	17	52	28	31	25	28·5	12	16	7·5	N.
1771.—Dec. 18,	21	7	35	43	58	6·0	16	25	20·2	N.

XIII. *New Metal discovered by M. Lampadius.*

Mr. Flor, Professor of Botany at Christiana, in Norway, states, in a letter, dated Nov. 28, to Dr. Müller, now in London, that M. Lampadius has lately discovered in some English ores (the characters of which are not mentioned), a new metal, which he calls *Wodanium.*

The same letter also says, that vegetation continued luxuriant around Christiana until Nov. 11, and that 70 species of wild plants continued in flower; and that many of those plants which are found exclusively in the regions of ice had blossomed a second time, but had since died away, the thermometer of Reaumur being three degrees above freezing point.

XIV. *Red Snow.*

This curious substance, which has so much attracted the public attention, is stated to have been found lying upon the surface of snow lodged in ravines for upwards of a hundred miles along the coast of Baffin's Bay. Considerable quantities were collected, and brought to this country in bottles, containing likewise the water of the snow upon which it had originally lain, as well as other substances apparently foreign, and having no connexion with the colouring matter. The following observations are founded upon experiments made upon minute quantities only, and are to be understood to apply to the colouring substance separated nearly from all foreign ingredients.

On opening the phial containing the substance diffused through the snow water, a very offensive odour, similar to that of putrid sea-weed, or excrement, was perceptible. After standing some time, the colouring matter slowly subsided, leaving the water colourless. When examined with a magnifier, it appeared to consist of minute particles, more or less globular, and of a brownish red colour. Separated and dried upon a filter, the red colour gradually disappeared, and was succeeded by a yellowish green hue. The smell also was different, and somewhat resembled train oil. It was insoluble in alcohol, caustic potash, and indeed in all other menstrua tried, even when assisted by heat. Nitric acid, assisted by heat, rendered it green; if concentrated, and in excess, this acid decomposed it entirely; and when the excess of acid was expelled by heat, a greenish yellow residuum, without the least trace of the pink hue afforded by lithic acid under similar circumstances, was obtained. Chlorine bleached it immediately.

When exposed to heat alone, it yielded a dense white smoke, which was very inflammable. The charcoal left, after incineration, afforded a very minute quantity of ashes, containing traces of lime, iron, and silex, the last two of which were probably extraneous.

From these observations, it is evident that this substance does not owe its colour and other properties to lithic acid, or oxide of iron. It seems, on the contrary, to be an organized substance; and the most general as well as probable opinion respecting its nature appears to be, that it is a production of some cryptogamous plant. The naturalist, therefore, will probably be better enabled to explain its origin and nature than the chemist.

From the circumstance of the red colour disappearing by exposure to the air, it seems to have undergone some change by keeping.

XV. *Sea Snake of America.*

Extracted from a letter from T. Say, Esq. of Philadelphia, to Dr. Leach:

"I have to regret that many of the scientific journals of Europe have taken serious notice of the absurd story which has originated to the eastward about the sea serpent; a story attributed here to a defective observation, connected with an extraordinary degree of fear. You have probably been informed that Capt. Rich has explained the whole business; he fitted out an expedition purposely to take this leviathan; he was successful in fastening his harpoon in what was acknowledged by all his crew to be the veritable sea serpent (and which several of them had previously seen and made oath to); but when drawn from the water, and full within the sphere of their vision, it proved to their perfect conviction that the sea serpent which fear had loomed to the gigantic length of 100 feet, was no other than a harmless Tunny (Schomber Thynnus) nine or ten feet long. Thus natural history is probably indebted to Capt. Rich for keeping from its pages an account of a second Kraken; and a memorable instance is added to the catalogue of credulity already pregnant with warning to naturalists."

ARTICLE XIV.

Magnetical and Meteorological Observations.

By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 42″ North. Longitude West in time 1′ 20·7″.

Magnetical Observations, 1818. — *Variation West.*

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Nov. 1	8h 25′	24° 40′ 17″	1h 35′	24° 43′ 11″	Owing to the shortness of the days, evening observation discontinued.	
2	— —	— — —	— —	— — —		
3	8 25	24 38 40	1 20	24 42 51		
4	8 20	24 39 22	1 15	24 42 38		
5	8 20	24 35 17	1 15	24 45 17		
6	8 25	24 39 29	1 15	24 42 00		
7	8 20	24 37 54	1 20	24 42 20		
8	8 30	24 37 34	1 45	24 42 58		
9	8 30	24 37 50	1 15	24 42 12		
10	8 20	24 36 17	1 20	24 44 04		
11	— —	— — —	1 20	24 42 21		
12	8 25	24 36 34	1 10	24 42 37		
13	8 30	24 39 02	1 15	24 49 18		
14	8 20	24 36 38	1 20	24 42 36		
15	8 20	24 33 20	1 20	24 36 50		
16	— —	— — —	1 05	24 37 15		
17	8 30	24 37 04	1 15	24 38 06		
18	8 25	24 35 22	1 10	24 39 19		
19	8 25	24 36 02	1 20	24 41 11		
20	8 25	24 36 45	1 25	24 41 12		
21	8 25	24 37 22	1 15	24 40 30		
22	8 25	24 38 09	1 15	24 40 41		
23	— —	— — —	1 15	24 41 06		
24	— —	— — —	1 15	24 40 42		
25	8 25	24 36 01	1 15	24 40 43		
26	— —	— — —	1 45	24 38 06		
27	8 30	24 35 35	1 15	24 45 45		
28	8 30	24 38 32	1 15	24 41 20		
29	8 30	24 36 40	1 35	24 39 44		
30	8 25	24 36 00	— —	— — —		
Mean for the Month.	8 25	24 33 24	1 19	24 41 41		

Meteorological Observations.

onth.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Nov.		Inches.				Feet.		
1	Morn....	29·490	49°	70°	W		Cloudy	48°
	Noon....	29·505	55	56	SSW		Cloudy	56
	Even	—	—	—	—		—	
2	Morn....	—	—	—	—		—	46½
	Noon....	—	—	—	—		—	47½
	Even	—	—	—	—		—	
3	Morn....	29·316	51	75	SSW		Fine	57½
	Noon....	29·285	57	53	SSW		Fine	57½
	Even	—	—	—	—		—	
4	Morn....	29·075	49	85	E by S		Foggy	47½
	Noon....	29·000	54	72	SSE		Showery	54
	Even	—	—	—	—		—	
5	Morn....	28·983	53	94	ENE		Rain	49
	Noon....	28·900	56	69	E		Cloudy	56½
	Even	—	—	—	—		—	
6	Morn....	28·885	53	70	E by S		Cloudy	52
	Noon....	28·918	55	60	E by S		Cloudy	58
	Even	—	—	—	—		—	
7	Morn....	29·220	51	78	S by W		Cloudy	50½
	Noon....	29·240	56	50	SSW		Very fine	56½
	Even	—	—	—	—		—	
8	Morn....	29·408	45	74	E		Very fine	43
	Noon....	29·429	53	58	E by S		Cloudy	52
	Even	—	—	—	—		—	
9	Morn....	29·500	44	90	ENE		Foggy	41
	Noon....	29·500	50	63	ENE		Fine	50½
	Even	—	—	—	—		—	
10	Morn....	29·502	45	82	E by N		Rain	44
	Noon....	29·465	48	58	ESE		Cloudy	48½
	Even	—	—	—	—		—	
11	Morn....	29·300	47	94	E		Rain	45
	Noon....	29·300	51	94	ESE		Fog, rain	51½
	Even	—	—	—	—		—	
12	Morn....	29·200	42	73	ESE		Fine	41
	Noon....	29·164	48	64	ESE		Cloudy	50
	Even	—	—	—	—		—	
13	Morn....	29·137	48	97	E		Foggy	42
	Noon....	29·150	56	65	SE		Very fine	56
	Even	—	—	—	—		—	
14	Morn....	29·155	53	81	S		Showery	48
	Noon....	29·105	55	75	SW by S		Showery	56
	Even	—	—	—	—		—	
15	Morn....	29·053	49	65	W by N		Showery	45
	Noon....	29·132	50	65	WNW		Showery	51½
	Even	—	—	—	—		—	
16	Morn....	29·173	49	97	SSW		Fog, rain	43
	Noon....	29·075	56	83	W		Rain	57
	Even ...	—	—	—	—		—	
17	Morn....	29·290	44	67	W		Very fine	43
	Noon....	29·355	51	50	WNW		Fine	51
	Even	—	—	—	—		—	
18	Morn....	29·580	39	66	WSW		Very fine	38
	Noon....	29·585	46	60	S by W		Cloudy	48
	Even	—	—	—	—		—	

Meteorological Observations continued.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Nov.		Inches.				Feet.		
19	Morn....	29·584	47°	68°	SSE		Cloudy	42°
	Noon....	29·590	51	66	SSW		Cloudy	51
	Even	—	—	—	—		—	37½
20	Morn....	29·454	39	67	ESE		Very fine	
	Noon....	29·395	46	52	E by S		Very fine	46½
	Even....	—	—	—	—		—	38
21	Morn....	29·263	39	63	ESE		Cloudy	
	Noon....	29·248	41	55	ESE		Cloudy	41
	Even	—	—	—	—		—	34
22	Morn....	29·259	35	60	E		Cloudy	
	Noon....	29·259	39	52	ESE		Cloudy	44
	Even	—	—	—	—		—	34½
23	Morn....	29·060	—	97	SE		Fog, rain	
	Noon....	29·080	53	74	SW by S		Showery	53
	Even ...	—	—	—	—		—	47
24	Morn....	29·268	—	97	ESE		Fog, rain	
	Noon....	29·300	50	92	W		Cloudy	50
	Even	—	—	—	—		—	37½
25	Morn....	29·650	38	83	S by W		Very fine	
	Noon....	29·663	47	60	S by W		Fine	49
	Even....	—	—	—	—		—	42
26	Morn....	29·664	—	90	SSW		Fog, rain	
	Noon....	29·670	51	80	SW		Fog, rain	54
	Even....	—	—	—	—		—	49
27	Morn....	29·920	51	87	SSW		Foggy	
	Noon....	29·936	54	63	WSW		Very fine	54
	Even....	—	—	—	—		—	49
28	Morn....	29·940	50	87	SSW		Foggy	
	Noon....	29·910	55	67	SW		Cloudy	55
	Even....	—	—	—	—		—	50
29	Morn....	29·859	52	94	SW		Foggy	
	Noon....	29·850	55	70	WSW		Showery	55
	Even	—	—	—	—		—	47
30	Morn....	29·770	50	71	WSW		Cloudy	
	Noon....	—	—	—	—		—	52
	Even	—	—	—	—		—	

Rain, by the pluviameter, between noon the 1st of Nov. and noon the 1st of Dec. 2·412 inches. Evaporation, during the same period, 1·08 inches.

ARTICLE XV.

METEOROLOGICAL TABLE.

		BAROMETER.			THERMOMETER.					
18.	Wind.	Max.	Min.	Med.	Max.	Min.	Med.	Hygr. at 9 a. m.	Rain.	
Mon.										
ɔv. 21	S E	29·70	29·67	29·685	40	37	38·5	64		☽
22	E	29·72	29·47	29·595	50	34	42·0	65	—	
23	S	29·70	29·47	29·585	54	43	48·5	92	27	
24	S	30·05	29·70	29·875	46	30	38·0	100	9	
25	S	30·14	30·05	30·095	50	32	41·0	82	10	
26	S W	30·32	30·10	30·210	54	48	51·0	93	13	
27	S W	30·40	30·32	30·360	54	48	51·0	88		
28	S W	30·38	30·30	30·340	57	48	52·5	76		○
29		30·30	30·20	30·250	58	46	51·0	75		
30	S	30·20	30·03	30·115	57	46	52·5	75		
Mon.										
ec. 1	S W	30·03	29·88	29·955	46	42	44·0	64	20	
2	N	30·00	29·60	29·800	48	36	42·0	71	—	
3	S W	29·60	29·45	29·525	49	35	42·0	100		
4	S W	29·42	29·37	29·395	47	40	43·5	76		☾
5	S E	29·57	29·42	29·495	51	33	42·0	74		
6	S E	29·58	29·27	29·425	51	40	46·0	77		
7	S W	29·65	29·27	29·460	54	44	47·0	81	30	
8	S E	30·00	29·65	29·825	54	36	50·0	88	10	
9	N E	30·10	30·00	30·050	46	32	39·0	76	40	
10	N E	30·13	30·08	30·105	45	32	38·5	88		
11	N E	30·14	30·08	30·110	43	30	36·5	71		
12	N W	30·12	30·07	30·095	42	37	39·5	80	2	●
13	N E	30·18	30·10	30·140	41	31	36·0	69		
14	N E	30·20	30·15	30·175	43	33	38·0	87		
15	N E	30·15	29·96	30·055	40	23	31·5	78		
16	N W	30·17	30·00	30·085	32	16	24·0	78		
17	N W	30·12	29·82	29·970	28	18	23·0	79		
18	Var.	30·00	29·70	29·850	39	25	32·0	84	13	
19	S W	30·25	30·00	30·125	43	29	36·0	86		
20	S W	30·10	29·90	30·000	47	43	40·0	73	1	☽
		30·40	29·27	29·925	58	16	41·20	79	1·75	

he observations in each line of the table apply to a period of twenty-four
n, beginning at 9 A. M. on the day indicated in the first column. A dash
tes, that the result is included in the next following observation.

REMARKS.

Eleventh Month.—21. Fair: cloudy, with a strong breeze. 22. *Cirri* tending to *Nimbus*, a. m.: *Cumulus* beneath *Cirrostratus*: little wind. 23. Wet, gloomy, a. m.: fair, p. m.: at sun-set, rose-coloured *Cirri*, with orange in the twilight. 24. Foggy morning: the dew frozen in the grass: the vane at SW. 25. A very dense *Cirrostratus*, a. m. forming a mist, which did not reach to the tops of the trees: a solar halo at 11: more clear in the evening: rain in the night: the wind SW to SE. 26. Wet, windy morning: fair and cloudy, p. m. and night. 27, 28. Cloudy. 29. Gloomy, fair, calm. 30. A breeze, with light clouds: fine, p. m. with *Cumuli*.

Twelfth Month.—1. Rain in the night. 2. The vane at N, a. m. but in the night the wind came to SW, blowing fresh, with a little rain. 3. Vane at S in the morning, with much wind: cloudy. 4. Fair, windy, cloudy. 7. A drizzling rain through the day. 6. Hoar frost. 8. Showery, a. m. 9. Wet. 10—20. Chiefly fair and cloudy: at intervals, fine, with the wind moderate: very white hoar frost on some of the latter mornings, with rime to the tops of the trees. Large lunar coronæ were frequent in the evenings, and lunar halo occurred more than once; but the dates were not noted.

RESULTS.

Winds Variable.

Barometer:	Greatest height	30·40 inches.
	Least	29·27
	Mean of the period	29·925
Thermometer:	Greatest height	58°
	Least	16
	Mean of the period	41·20
Mean of the Hygrometer		79
Evaporation		0·35 inches.
Rain		1·75 inches.

The few nocturnal frosts that occurred in the present season up to the middle of the month were so slight as to permit the *Nasturtiums* (the tenderest of our autumnal garden flowers) to continue to vegetate: other indications of the mildness of the season were equally striking. I observed a horse-chesnut with tufts of new leaves and blossoms put forth from the ends of the branches all over the tree; but the severe nights, and some frost by day, since the 15th, have put a seasonable stop to vegetation. The temperature of the latter half of the period, and the hygrometer throughout, were noted at the laboratory.

TOTTENHAM, *Twelfth Month*, 22, 1818. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

FEBRUARY, 1819.

ARTICLE I.

Short Account of the Scientific Writings of Dr. Ingenhousz. By Thomas Thomson, M.D. F.R.S.

In volume x, p. 161, of the *Annals of Philosophy*, there is inserted a biographical account of Dr. Ingenhousz, in which Dr. Garthshore, the writer of the article, has given a short account of the writings of this ingenious philosopher. Probably the readers of the *Annals* will not be displeased if I take a short review of such of Dr. Ingenhousz's papers as I have had an opportunity of reading, and endeavour to point out the particular scientific discoveries, or improvements, for which we are indebted to him.

1. His first paper *on the torpedo* was published in the Philosophical Transactions for 1775. It merely informs us that being in Leghorn in December, 1772, he went out 20 miles to sea, and caught a number of torpedoes. He verified the power which this fish has of giving electric shocks. These shocks he found very weak, which is usually the case in winter, and they could not be communicated through a chain of metal.

It was in 1773 that Mr. Walsh made his celebrated observations and experiments on torpedoes. These experiments had been published before Dr. Ingenhousz's paper, and of course had anticipated all the facts contained in it. But it would appear from a comparison of dates, that Dr. Ingenhousz's experiments were made at least as early, if not earlier, than those of Mr. Walsh. The subsequent experiments of Mr. Cavendish, and the comparison of the electrical organs of the torpedo with those of the voltaic battery, have thrown much additional light on the electrical powers of this and several other similarly endowed fishes, and have quite thrown into the shade the few facts communicated by Dr. Ingenhousz in this paper.

2. *Easy Methods of measuring the Diminution of Bulk taking place on the Mixture of Common and Nitrous Air; with Experiments on Platina.* Phil. Trans. 1776, p. 257.—Dr. Ingenhousz employed Fontana's eudiometer in his experiments on the diminution produced by mixing common air and nitrous gas; and he describes in this paper several ingenious methods which he had recourse to in order to determine more accurately the bulk of the two gases before mixture. But though Dr. Ingenhousz appears to have bestowed great attention upon this mode of determining the purity of air, and though he continued his experiments for a long series of years, chemistry derived very little advantage from the result of his researches. His notion of the nature and constitution, both of common air and nitrous gas, was inaccurate, and of course his opinion of the cause of the diminution of bulk which takes place when they are mixed was equally erroneous. Besides, he was not aware of the many circumstances which produce variations in the condensation even when the state of the two gases before mixture is precisely the same. The first person that pointed out the method of making this experiment with the requisite accuracy was Mr Cavendish, in a paper on a New Eudiometer, published in the Phil. Trans. for 1783. He showed in that paper that the purity of common air does not vary at different seasons of the year, and in different places, as had been previously supposed; but that when the experiment is correctly made, we find its purity, or the proportion of its constituents, always exactly the same. It further appears from Mr. Cavendish's experiments, that when common air is let up into nitrous gas, bubble by bubble, agitating the nitrous gas, during the whole time, over water, that oxygen combines with, and condenses almost exactly, four times its volume of nitrous gas. Much pains have been taken by some of the most ingenious chemists of the present day, particularly by Mr. Dalton and M. Gay-Lussac, to determine how much nitrous gas is capable of uniting with oxygen gas. But I consider Mr. Cavendish's determination of the maximum of nitrous gas as fully as accurate as any of the subsequent experiments. It seems to be nearly agreed upon that the minimum proportion of nitrous gas is one volume of oxygen gas and $1\frac{1}{3}$ of nitrous gas. This proportion was first, I believe, hit upon by Sir. H. Davy.

Dr. Ingenhousz's observations on platina are of little value. He found the grains of native platina attracted by the magnet. He could not melt them; but on passing an electric shock through a parcel of these grains put into a small glass tube, he cemented them together. We now know that the magnetical properties of the grains of platina are owing to the presence of iron with which that metal is alloyed. Platinum in a state of purity is not in the least attracted by the magnet.

3. *A ready Way of lighting a Candle by a very moderate Electrical Spark.* Phil. Trans. 1778, p. 1022.—Dr. Ingenhousz, if we are to form our opinion of him from his writings, seems to have been rather attached to parade and show, and probably took great delight in exhibiting brilliant experiments to others. Some of the most showy experiments (if they be entitled to the name), still exhibited in chemical lectures, were contrived by him; such as the combustion of iron wire, of camphor, and of phosphorus in oxygen gas, and the inflammable air pistol by means of common air and ether. The experiment described in this paper may, perhaps, without much impropriety, be classed along with those just mentioned. He kept by him a ready charged Leyden jar, the knob of which was bent a little so as rather to hang over the jar. He had likewise an assortment of brass wires of the requisite length with a little cotton tied loosely at one end of each. This cotton was dipped into finely powdered resin. One end of the wire being brought in contact with the outside coating of the jar, the other extremity to which the cotton was attached was brought near the knob. A spark was discharged, which set fire to the cotton, and by means of this flame, which lasted about half a minute, it was easy to light a candle.

4. *Electrical Experiments to explain how far the Phenomena of the Electrophorus may be accounted for by Dr. Franklin's Theory of Positive and Negative Electricity.* Phil. Trans. 1778, p. 1027.—This is, perhaps, the most valuable of all the scientific papers of Dr. Ingenhousz inserted in the Phil. Trans. It explains the phenomena of the electrophorus in a very clear and satisfactory manner. The electrophorus is a very curious and useful instrument, invented by Volta. It consists essentially of a cake of resin covered with a plate of metal, moveable at pleasure by means of a glass handle attached to it. When the cake of resin has been charged with electricity by means of a Leyden jar, if you put the metallic plate over it, and while in that position touch the upper part of the plate with your finger, on lifting up the plate by means of its glass handle, it will be found charged with the opposite kind of electricity of the resinous cake, and will give a spark to any conductor brought into its neighbourhood; and this experiment may be repeated at pleasure for months together without any renewal of the charge of the resinous plate.

Dr. Ingenhousz's explanation depends upon two principles, which, he says (at least the first of them), had not been attended to before his time. These principles are the following: 1. Electrical bodies do not easily receive an electrical charge; but when once charged, they are not easily deprived of the electricity thus communicated. The consequence is, that they in general retain it for a long time; except glass, which from its property of attracting moisture, is speedily deprived of any charge that may be communicated to it. Hence the plate of resin of the electrophorus, when once excited, retains its charge for a long time, and does not communicate any of it to the metal plate, though laid upon it. When a conductor is brought into the vicinity of an excited electric, the side of it next the electric acquires the opposite kind of electricity of that of the electric, and the furthest off side becomes in the same state as the electric. Suppose the cake of resin charged positively, the side of the metal plate next the resin will become negative, and the opposite side positive; because the electricity of the resin repels the electricity of the metal, and drives it to the side furthest from itself. If we now touch the side of the metal plate furthest from the resin, it will discharge its surplus electricity into our body. When the plate is removed from the resin, the electrical fluid will spread itself equably through it; but as it has parted with a portion of its electricity to our body, it must contain less than its usual portion, and of course be negative. A conductor brought into its neighbourhood will of consequence transmit a spark to it, and restore the usual quantity of electricity to the metallic plate. This experiment may be repeated at pleasure, because the cake of resin merely alters the distribution of the electricity of the plate. Our touching the plate takes away or communicates a quantity of electricity, according to circumstances. Hence, when removed, it is constantly excited, and in a condition to give out or receive a spark.

Such is Dr. Ingenhousz's explanation of the electrophorus. I rather think that the first of the principles upon which this explanation is founded was new; at least I am not aware of any person who advanced it explicitly before our philosopher. But the second principle was not new, having been advanced by Dr. Franklin, and mathematically explained by Mr. Cavendish and Epinus. Dr. Ingenhousz does not seem to have possessed mathematical knowledge, and, therefore, was not likely to have perused the theory of electricity as given by Cavendish in his celebrated paper on that subject, or by Epinus in his well-known work; but it is not at all likely that he was ignorant of the principles advanced by Dr. Franklin. Accordingly Dr. Ingenhousz does not claim the second principle as a discovery of his own. He merely makes use of it to show that the phenomena of the electrophorus were not inconsistent with the Franklinian theory, but really followed from that theory.

5. *Account of a new Kind of Inflammable Air, or Gas, which can be made in a Moment without Apparatus, and is as fit for Explosion as other Inflammable Gases in Use for that Purpose; with a new Theory of Gunpowder.* Phil. Trans. 1779, p. 376.—This paper merely makes us acquainted with the fact that ether explodes when the vapour of it is mixed with common air, or

oxygen gas; and that only a small proportion of vapour must be employed for the purpose. If we use too much, no explosion takes place at all. The combustion of ether has been investigated by more modern chemists with considerable precision. Mr. Cruikshanks made some progress in the investigation; Mr. Dalton went further; and M. Theodore de Saussure has obtained results which approach to accuracy; so that but little remains in order to complete this important subject. Dr. Ingenhousz must be admitted to have begun the investigation, though he advanced no further than merely the discovery of the detonating power of vapour of ether when mixed with oxygen gas.

But this paper contains some historical facts which I consider as rather interesting. They seem to have been in general overlooked by chemists. The person who discovered the fact that oxygen and hydrogen gases may be exploded by means of an electric spark was Sir William Watson; undoubtedly, one of the most distinguished electricians which England at that time, so rich in first rate proficients in that science, possessed. This discovery, trifling as it may appear, deserves to be remembered; because it has contributed so essentially to the progress of an accurate mode of examining gaseous bodies.

The first persons who collected olefiant gas were Messrs. Æneæ and Cuthbertson. This they did at least as early as 1777, by heating a mixture of equal quantities of sulphuric acid and alcohol. Mr. Æneæ ascertained the specific gravity of this gas, its combustibility, and the colour of the flame. It was known, therefore, though it had not been very accurately examined before the experiments of the Dutch chemists in 1794.

Dr. Ingenhousz was of opinion that when gunpowder was heated, the nitric acid of the saltpetre gave out oxygen gas, while the charcoal gave out inflammable gas, and that the explosion was owing to the instantaneous combustion of this mixture. It would be needless to make any observations on this theory, as it is now known to be inaccurate in every particular.

6. *On some new Methods of suspending Magnetic Needles.* Phil. Trans. 1779, p. 537.—Dr. Ingenhousz balanced the needles so that they were under the surface of water, and conceived that by this method he succeeded in obviating most of the irregularities in the motions of needles. It is obvious that such a method is quite inapplicable to the use of the needle on shipboard, which is the great purpose for which it is applied. Nor is it at all likely that plunging a needle under water would make it answer better for philosophical purposes.

7. *Improvements in Electricity.* Phil. Trans. 1779, p. 659.—This paper was the subject of the Bakerian lecture for 1779. It consists in a historical detail of the progress which the plate glass electrical machines had made. Dr. Ingenhousz informs us that he had suggested this kind of machine about 15 years

before. He relates the various improvements made in these machines in France and Italy, and particularly by Mr. Cuthbertson at Amsterdam, who brought that kind of machine to perfection, and still continues to make them in Poland-street, London.

8. *On the Degree of Salubrity of Common Air at Sea, compared with that of the Sea Shore, and that of Places far removed from the Sea.* Phil. Trans. 1780, p. 354.—He tried air by Fontana's method at the mouth of the Thames, and afterwards at Ostend, and in various parts of the Netherlands, France, and Germany. He concluded from his observation, that the air is purer at sea than over land, and purer on the sea shore than at a distance from the ocean. But these inferences were made from too limited a number of experiments. Indeed only one, or at most two experiments were made at sea. It is now perfectly established that there is no difference whatever between air at sea and air at land. The supposed differences originated entirely from inaccuracies in the mode of making the experiments, and disappeared as soon as chemists fell upon accurate methods of analyzing common air.

9. *Experiments upon Vegetables, discovering their great Power of purifying the Common Air in the Sunshine, and of injuring it in the Shade and at Night. To which is joined a new Method of examining the accurate Degree of Salubrity of the Atmosphere.* London, 1779.—This is an octavo volume of 302 pages, which Dr. Ingenhousz published in 1779, just before setting out for the continent. He gives an account in it of a set of experiments which had occupied him incessantly for about three months during the preceding summer. Only two points are established in this book. 1. That the leaves of plants give out oxygen gas when exposed to the sun under pump water. 2. That the proportion of oxygen in the air immediately in contact with plants is diminished during the night. But whether this is owing to the absorption of oxygen, the emission of carbonic acid, or the conversion of the oxygen into carbonic acid, is not ascertained. Dr. Ingenhousz, at the time he made his experiments, had no accurate ideas respecting the composition of air, nor respecting the action of nitrous gas on air. He was not aware of the different nature of hydrogen and carburetted hydrogen gas, and perpetually confounds them together. M. Theodore de Saussure's experiments on this subject are much more precise; though even he has not thrown so much light upon it as is to be wished. Dr. Ingenhousz in this case, as in his explosions with common air and ether, had the merit of commencing the investigation; but his progress in it was very small.

The method of determining the goodness of air, described in this book of Dr. Ingenhousz, is merely the Abbe Fontana's, a little abridged, and throws no additional light upon the constitution of air.

10. *Some further Considerations on the Influence of the Vegetable Kingdom on the Animal Creation.* Phil. Trans. 1782, p. 426.—Our author's opinion that vegetables emit oxygen gas when growing in the sun, and that the injury done to the air by the breathing of animals and by combustion is in this way repaired, having been called in question by some persons, and it having been alleged that it was altogether refuted in Dr. Priestley's fifth volume of experiments on air, Dr. Ingenhousz made a public exhibition of the most decisive of his former experiments to a number of his scientific friends. He boiled pump water for two hours to deprive it of its air. It was then put into glass vessels placed inverted over mercury, to shut out the communication with the atmosphere. Some conferva rivularis was put into two of these glasses, some pieces of cloth into other two, and nothing in other two. The first two glasses began in three days to yield oxygen gas, which on examination proved very pure, and the conferva gave out altogether about eight times its bulk of this gas. In 10 days, it ceased to vegetate, and began to decay. The cloth gave out no air whatever; neither was any air collected in the jars into which nothing had been put even at the end of some months. A glass containing pump water unboiled began to yield air much sooner, and it yielded a greater quantity; but it was not so pure.

11. *Nouvelles Experiences et Observations sur divers Objets de Physique.* A Paris, 1785.—This is chiefly a French translation of the papers of Dr. Ingenhousz, already printed in the Phil. Trans. He was induced, he tells us in the preface, to translate them himself, because all the translations which he had seen contained mistakes which materially altered his meaning. In his own translation, he was enabled to rectify these mistakes; and he likewise added some additional illustrations, which he thought likely still further to elucidate the subject. It will only be necessary, therefore, to notice the papers which made their first appearance in this octavo volume of 498 pages.

The first paper is an outline of the Franklinian theory of electricity; very short; but clear and precise; and exhibited without any mathematical phraseology. In this respect, he followed the example of Dr. Franklin himself.

The second paper is a theory of the electrophorus, more detailed than the paper on the same subject in the Transactions, but quite the same in point of theory.

The third paper is of some length, and consists of a set of observations on a question at that time agitated with great keenness in England; whether thunder rods ought to terminate in points, or round knobs. The question had originated in the gunpowder magazine at Purfleet having been struck with lightning. A committee of the Royal Society had been appointed to investigate the subject. From the report of this committee, Mr. Wilson dissented. He affirmed that the extremity of thunder rods should be blunt. He exhibited a set of experiments in support of his opinion in the Pantheon, at which his present Majesty attended. The King adopted the opinion of Mr. Wilson, and altered in conformity with it the thunder rods attached to St. James's Palace. But Mr. Wilson made no other convert: the dispute continued for a considerable time; but seems to have terminated in favour of pointed thunder rods. Dr. Ingenhousz in this paper gives his reasons for considering pointed rods as the most proper in conformity with the original proposal of Dr. Franklin; and his reasons appear perfectly satisfactory. It seems unnecessary to state them here, as I am not aware that any person at present supports the doctrine of Mr. Wilson.

The fourth paper is a description of an electrical machine used by our author for various purposes. It consisted of a piece of strong silk suspended against the wall of a room, to which was attached a rubber of hare skin, or cat skin. He used it for charging small Leyden jars of a peculiar construction, which he describes.

In the fifth paper he describes a small pocket electrical machine, intended for producing a spark sufficient for firing an inflammable air pistol.

In the sixth, he gives a description of the mode of burning camphor and phosphorus in oxygen gas, and describes the brilliancy of these experiments with rapture.

In the seventh, we have a contrivance for producing a vacuum by the property which charcoal has of absorbing air. It is needless to observe that there seems little probability of any such substitute for an air pump being of much utility.

In the eighth paper, we have a description of a method of lighting a candle by means of an electrophorus and an inflammable air lamp. The invention belongs to Strasburg. At present we are in possession of better methods of accomplishing this object than those described in this paper.

In the ninth paper, he describes his inflammable air pistol. This contrivance is too well known to require any details here.

The tenth paper is a description of the mode which he employs to collect carburetted hydrogen gas from the bottom of stagnant water.

The eleventh and twelfth papers are translations from the Transactions.

The thirteenth paper is on oxygen gas. He procured it from saltpetre, and he conceives that it would be a medicine of very great efficacy if it were given to patients ill of certain diseases, to be respired instead of common air. This opinion has not been verified by subsequent trials.

The fourteenth paper is on the salubrity of the air above the sea compared with the air at dry land. It is a translation from the Transactions.

In the fifteenth paper he gives us an account of some attempts make artificial magnets after the manner of Dr. Gowan Knight; t these attempts were not very successful.

In the sixteenth paper, he gives his theory of gunpowder. He patiates at greater length; but the theory is precisely the me as that which he had already given in the Transactions.

The seventeenth paper is an application of the same theory to lminating powder.

The eighteenth paper is one of the most valuable in this volume. consists in a set of experiments made to determine which of e seven metals, gold, silver, copper, tin, steel, iron, and lead, nducted heat best. The mode of making the experiment was ntrived by Dr. Franklin, who likewise supplied the materials. ires of each of these metals of the same length and thickness re coated with wax, and their ends dipped into boiling water. e wire on which the wax was melted and highest up was ckoned the best conductor. Silver was found the best con- ctor, and lead the worst in all the experiments. Copper was e next best conductor, gold the next best, tin, steel, and iron, xt best.

In the last paper contained in this volume, Dr. Ingenhousz scribes his mode of burning iron wire in oxygen gas, and his tempts to burn the other metals in the same gas. These last tempts, except with platinum wire, had not been successful.

These are the only writings of Dr. Ingenhousz which I have ad the opportunity of perusing. They contain, I believe, all he additions of any importance which he made to chemistry r electricity. His turn of mind did not lead him so much to he investigation of the properties of bodies as to the discovery f what he considered to be striking or brilliant; and having got something of this kind, he seems often to have remained satisfied without any attempt to investigate what actually happened during the experiment. Thus the analysis of the combustion of iron wire in oxygen, and of the vapour of ether in oxygen, he left to Lavoisier and Cruikshanks, who, by investigating them with care, established important theoretical points in the science of chemistry, and thus contributed materially to improve it.

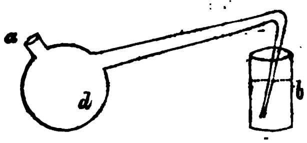
ARTICLE II.

Contributions towards the History of Anthrazothionic Acid, discovered by Porrett, and called by him Sulphuretted Chyazic Acid. By Theodor von Grotthuss.

(*Concluded from* p. 50.)

Sect. 17.—By this stochiometrical analysis, we find the proportion of water in the copper anthrazothionhydrate determined in sect. 14 by an empirical way very completely confirmed; for the eighth part of a hundred is 12·5; and our number comes out 12·63, which is so near a coincidence that we may consider the experimental and the calculated numbers to coincide. It appears also, that the view of the subject stated in a former section as probable, corresponds correctly with matter of fact; for the hydrogen of the acid added to $\frac{4}{5}$ths of the oxygen of the oxide gives a quantity of water amounting exactly to $\frac{1}{8}$th of the whole weight of the anthrazothionhydrate. We shall see likewise below how exactly the hydrogen of the anthrazothionic acid, derived from the same view (namely, 0·067 hydrogen in 1·510 acid), corresponds with the same constituent obtained in quite another way. Every hypothesis ceases to be a mere hypothesis when the phenomena connected with it, being subjected to mathematical calculation, correspond exactly, not only with each other, but with the hypothesis itself. It may then be considered as nothing else but a bare statement of a set of facts, and is raised to the rank of that highly scientific term—*a theory*.

Sect. 18. *Experiments to determine the Constituents of Anthrazothionic Acid.*—I got blown in a glass-house a number of small retorts, of the shape *d*, terminated by a long bent tube, and having a mouth, *a*. These retorts I find very convenient for small chemical experiments. Through the mouth, *a*, of one of these retorts, I introduced a portion of anthrazothionate of potash in a crystallized state, and poured over it a concentrated solution of chlorine. Immediately the mouth, *a*, was shut, and the extremity of the bent tube introduced into the vessel, *b*, containing lime water. Chemical action immediately began, and it was accelerated by applying heat below the retort, *d*. The phenomena which took place were effervescence, a considerable precipitation of sulphur in the retort, *d*, and the evolution of bubbles of gas which rendered the lime water in the vessel *b* milky.* After the solution had been made boiling hot, and the chemical action appeared over, I collected the whole of the precipitated sulphur upon a filter, the weight of which had been previously determined. It was washed carefully clean by repeated affusions of water, exposed for some days to the summer temperature of the air, and finally dried upon a warm plate. In this state I found that the filter had increased in weight 2 gr. This increase was wholly owing to the presence of pure sulphur, easily recognisable by its properties. The filtered liquid contained a portion of sulphuric acid formed dur-



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* The vessel *b* must be rather long, and it must be filled with lime water; and the bent glass tube which terminates the retort ought to terminate in a very narrow mouth, that the lime water may come in contact as much as possible with every part of the evolved gas.

g the chemical process; for muriate of barytes threw down a recipitate in it, which, when collected, washed, dried, and heated o redness in a watch-glass upon Guyton's lamp apparatus, veighed 4·1 gr. To this must be added $\frac{1}{10}$th gr. of the precipitate vhich remained behind upon the filter; for the weight of the ilter was increased by a little more than $\frac{1}{10}$th grain; but I estrict it to that quantity on account of the moisture which could not be completely dissipated on the filter. Hence we may reckon the weight of the sulphate of barytes formed 4·2 gr. which is equivalent to 0·57 gr. of sulphur. Of consequence the quantity of sulphur obtained by the decomposition of the anthrazothionic acid amounted to 2·57 gr.

Sect. 19.—The precipitate which was formed in the lime water being collected, washed, dried, and weighed, was found to amount to 2·85 gr. Diluted acetic acid dissolved it with effervescence, carbonic acid gas being evolved; but there remained behind a very small quantity of a white powder, which after being washed and dried could not be weighed. A drop of sulphuric acid being let fall upon it, the odour of sulphurous acid became evident. Hence it appears that a little sulphite had been formed at the same time with the carbonate; so that sulphurous acid had been given out as well as carbonic acid. As the acetic acid and the edulcorating water must have dissolved a portion of this sulphite, we cannot err very much if we consider it as equal to 0·11 gr. This being subtracted from the weight of the carbonate, leaves 2·74 gr.; but 0·11 gr. of sulphite of lime are nearly equivalent to 0·03 sulphur. Hence the sulphur in the portion of acid subjected to experiment was 2·57 + 0·03 = 2·6 gr.; and 2·74 gr. of carbonate of lime are an equivalent for 0·328 gr. of carbon. It follows from this, that in anthrazothionic acid the sulphur bears to the carbon the proportion of 2·6 : 0·328. It was the object of the preceding experiment to find this ratio.

Sect. 20.—Into a glass tube hermetically sealed at the under end, I put a small portion of dry crystallized anthrazothioniate of potash, and filled up the rest of the tube, except a very small space, with mercury. The friction against the sides of the tube prevented the salt from leaving the bottom and being buoyed up to the top of the tube. By agitation and by the proper application of heat, I at last drove every trace of atmospherical air out of the tube. I now filled the residual portion of the tube with sulphuric acid, shut its mouth with the finger, and, turning it upside down, introduced it into a vessel filled with mercury, and then removed the finger. The sulphuric acid, from its liquidity, and the greater specific gravity of the mercury, must of course make its way to the upper part of the tube, and come in contact with the salt. Heat being applied to the outside of the tube, it was at last almost completely filled with gas extricated from the decomposed anthrazothionic acid, so that only a few drops of mercury remained in it, over which floated the magma composed of the acid and the salt. The tube was now shut, removed from the mercury, washed on the outside with water, and plunged into a glass vessel filled with distilled water, A, in order to free it from the small quantity of mercury which it contained, and from the acid magma which floated over the mercury. These being removed, the mouth of the tube was again shut, and it was introduced into lime water, and left in it. The lime water made its way visibly into the tube, and became quite milky, and the tube, being agitated, was in about 15 minutes completely filled, so that not even a trace of gas remained unabsorbed (for a residue of azote might have been expected). The precipitate which fell in the lime water consisted of carbonate and sulphite of lime. The lime water had likewise absorbed undecomposed anthrazothionic acid, and struck a yellowish red colour with a solution of iron. The distilled water A contained not only sulphuric acid and a portion of precipitated sulphur, but likewise *sulphate of ammonia;* for being heated and mixed with an excess of potash ley, it emitted a strong smell of ammonia. This experiment was several times repeated, and always gave the same result. It follows from it obviously that *anthrazothionic acid either contains ammonia as a constituent, or at least hydrogen and azote in the exact proportion requisite for forming ammonia.*

Sect. 21.—I have in vain attempted by a similar mode of decomposition to separate the azote in a gaseous form from anthrazothionic acid. When I put anthrazothionhydrate of copper mixed with chlorate of potash into the tube, and added muriatic acid to the mixture, I obtained, it is true, from 1 gr. of the hydrate, after the other gases had been absorbed, a quantity of azotic gas, which, at the common temperature of the atmosphere, and when the barometer stood at 30 inches, was equivalent to 18·5 gr. of water; but muriate of ammonia was found in the residual sour liquid, and by calculation, founded on the subsequent stochiometrical construction, I found that only $\frac{1}{3}$d of the azote in the acid had been evolved in the gaseous state. The constituents of this acid might be most easily and accurately ascertained by means of Volta's eudiometer. But for this purpose not merely a convenient mercurial apparatus is requisite, but likewise an eudiometer attached to the mercurial trough. Now as I happen not to possess any such, I am under the necessity of employing a more tedious, but not less accurate method of determining these constituents.

Sect. 22.—We have now found two facts which are of importance towards the determination of the constitution of anthrazothionic acid; namely, the ratio of the sulphur to the carbon, and that of the azote to the hydrogen. Now to find the ratio of one of the former of these bodies to one of the latter, by which the ratio of all the four constituents to each other is ascertained, and consequently the analysis of the acid completed, nothing more is requisite than to determine the absolute weight of one of the four constituents in a given quantity of anthrozothionic acid. For this purpose, I made choice of the sulphur, because its weight, when it is converted into sulphuric acid, may be determined with very great precision. Porrett has already, it is true, determined the proportion of sulphur in anthrazothionic acid; but as my analysis of anthrazothionhydrate of copper, derived from Porrett's experiments and my own observations, does not agree with that of Porrett, I consider it as necessary to repeat Porrett's experiments in a different way.

Sect. 23.—Five grains of white anthrazothionhydrate of copper prepared from acetate of copper and an alcoholic solution of anthrazothionate of potash, after being well dried for some days in a heat not sufficient to alter its colour, was well mixed with 25 gr. of chlorate of potash, and then covered with concentrated muriatic acid containing a saturated solution of muriate of barytes. The glass cup in which this mixture was put was covered with a glass plate, in order to prevent any of the liquid from being driven out of the glass by the effervescence, and the cover was kept applied till all chemical action was at an end, even when it was assisted by a strong heat. I now added an additional portion of muriate of barytes and some chlorate of potash, and allowed the liquid to digest till the portion of sulphur which had escaped oxidation was converted into sulphuric acid and united with barytes. During the whole process I could perceive no smell of sulphurous acid, but a strong one of chlorine, of which a great superabundance was present. The sulphate of barytes being collected and dried weighed 8·1 gr. which approaches very nearly to the quantity obtained by Porrett.

But we have already (sect. 16) ascertained that 4·58 gr. of anthrazothionhydrate of copper contain 1·510 gr. of anthrazothionic acid. Of course, five gr. of the hydrate must contain 1·65 gr. of the acid. Now 8·1 gr. of sulphate of barytes are equivalent to 1·11 gr. of sulphur. From this it follows that 100 gr. of the acid contain 67·3 parts of sulphur. This comes very near the estimate of Porrett, according to whom the acid contains ⅔ds of its weight of sulphur.

Sect. 24. *Stochiometrical Estimate.*—Thus we have found the third requisite for constructing a stochiometrical synthesis of the acid. From these three data; namely, 1. That in the acid the sulphur is to the carbon as 2·6 to 0·328; 2. That the azote and hydrogen exist in it in the same proportions as in ammonia; and, 3. That the acid contains 67·3 per cent. of sulphur: it is easy to give a stochiometrical statement of the synthesis and analysis of this acid. In 100 parts of anthrazothionic acid there are 67·3 of sulphur: the remaining 32·7 consist of the other three constituents of the acid; and the proportion of carbon to that of sulphur is as 0·328 to 2·6. Now 2·6 : 0·328 :: 67·3 : 8·49, which, for the sake of shortness, I shall make 8·5. Thus the quantity of carbon in 100 acid is 8·5. Finally, the remainder = 32·7 − 8·5 = 24·2 consist of azote and hydrogen in the same proportion as they exist in ammonia. Now, according to Wollaston's scale of equivalents, 21·52 ammonia contain 17·54 of azote. Of consequence, the 24·2 parts must consist of 19·7 azote and 4·5 hydrogen.

Of course the constituents of 100 parts of anthrazothionic acid are composed of

Sulphur	67·3
Carbon	8·5
Azote	19·7
Hydrogen	4·5
	100·0

Or, according to the atomic theory,

3 atoms sulphur	= 60·00
1 atom carbon	= 7·54
1 atom azote	= 17·54
3 atoms hydrogen	= 3·98
	89·06

Sect. 25.—We may consider this acid either as a compound of *sulphuretted carbon* (= 3 atoms sulphur + 1 atom carbon) and *ammonia* (= 3 atoms hydrogen + 1 atom azote); or of *sulphuretted hydrogen* (= 3 atoms sulphur + 3 atoms hydrogen) and *carburetted azote* (= 1 atom carbon + 1 atom azote). But we must not confound *cyanogen* under this carburetted azote; for it contains twice as much carbon as our compound; and on that account it might, by way of distinction, be called *carbonized carburetted azote*. The carburetted azote observed by Fourcroy (Ann. de Chim. xi. 45) may, perhaps, be a compound of one atom of carbon with one atom or two atoms of azote. From this statement of the constituents of anthrazothionic acid, it follows that in all the experiments of Porrett in which he made cyanogen or prussic acid to act upon sulphuretted hydrogen and potash, the half of the carbon contained in the cyanogen, while he was converting it into anthrazothionic acid by means of sulphuretted hydrogen, must have made its escape either as carbonic acid gas, or carburetted hydrogen gas, or in some other way. This would deserve to be investigated hereafter with the requisite precision. Meanwhile, it is easy to see that when anthrazothionic acid is decomposed by means of an oxidizing medium, neither cyanogen nor hydrocyanic acid can be formed; because the oxidation will first act upon the carbon, on account of its being more oxidizable than the azote; and, of course, the ratio of the former to the latter will always be diminishing.

Sect. 26.—We have it now in our power to prove the truth of the assumption made in sect. 15 with great probability indeed, out still in an arbitrary manner; namely, that the hydrogen of the anthrazothionic acid, when it forms an anthrazothionhydrate of copper with the oxide of that metal in solution, deprives the oxide of only $\frac{4}{5}$ths of its oxygen, while the remaining $\frac{1}{5}$th unites with the disoxygenizing medium, the presence of which is necessary. This we can do by making a stochiometrical calculation of the constituents of anthrazothionic acid founded on that assumption, and comparing it with the result which we obtained in sect. 24. When we calculated the constituents of anthrazothionhydrate of copper (sect. 16 and sect. 17), we found that 1·51 of anthrazothionic acid, *following the above-mentioned assumption*, must contain in itself 0·067 of hydrogen. On that occasion I omitted the figure in the fourth decimal place altogether; and to make the calculation more easy, I did not hesitate to admit an error of one or two unities in the third decimal place. But the accurate quantity of hydrogen which 1·51 of the acid contains is 0·0678; for 10 oxygen requiring 1·325 hydrogen, it follows that 0·512 oxygen (namely $\frac{4}{5}$ of 0·64) must require 0·0678 of hydrogen; but if 1·51 of acid contain 0·0678 hydrogen, 100 of acid must contain 4·49 of the same element. Now this differs only by one unity in the second decimal place from the number found in sect. 24 by a very different process. This exact coincidence leaves no doubt about the truth of our assumption. Were we to complete the reckoning by the application of the data obtained in sect. 24 for the other constituents, we should obtain the same, or very nearly the same numbers. Of consequence the *existence of anthrazothion*, at least in combination with the easily reducible metals by the exact agreement of the two modes of calculation, is placed *beyond all doubt.*

Sect. 27. *The Constituents of Anthrazothionic Acid determined stechiometrically according to the Theory of Volumes.*—In order to be able to transfer the constitution of our acid to the theory of volumes, which seems best adapted to exhibit a clear view of the composition of bodies as free as possible from all hypothetical assumptions, it will not be improper to lay the following observations before the reader, in the first place, by way of introduction.

The theory of volumes is founded on the assumption that bodies unite with each other in the state of gas, and in definite proportions. Suppose then that the weight of a determinate volume of atmospherical air at the temperature of 32° and under a pressure of 30 inches of mercury, be reckoned = 1·000, and that the absolute weights of all other bodies in the state of gas, and under the same circumstances, be ascertained. These absolute weights exhibit at the same time the specific gravity of each body, referred to that of atmospherical air as unity. The specific gravity of permanently elastic gases can be determined by the well-known method of weighing a determinate volume of each at a given temperature, and under a given barometrical pressure.

But to be able to determine the specific gravity = x of a substance supposed in the gaseous state, which is not capable of existing alone in that state, we must, in the first place, endeavour to ascertain the specific gravity = A of a gaseous combination of this substance with another gas, of which last the specific gravity = B is already known. Further, we must know the number of volumes of each which are requisite to form one volume of the compound gas, whose specific gravity is = A. Let a be the number of volumes of the body whose specific gravity = x is required, and let b be the number of volumes of the gas whose specific gravity = B is already known: it is evident that $a\,x = A - b\,B$, and, of course, $x = \frac{A - b\,B}{a}$.

Sect. 28.—An example or two will be sufficient to elucidate this rule. From the experiments of Davy and Gay-Lussac, it is known that hydrogen and oxygen gases do not alter their volume when they combine with sulphur, and are converted respectively into sulphuretted hydrogen and sulphurous acid gases. We may, therefore, assume it as very probable, and as conformable to the law of gaseous combinations, that one volume of sulphur in the state of gas unites with one volume of hydrogen or oxygen gases; and that the two volumes in both cases are condensed into one volume. Let us apply our formula in this case where $a = 1$, and $b = 1$. The specific gravity of a volume of sulphur in the state of gas will be $x = \frac{A - 1b}{1} = A - B$. According to Thomson, the specific gravity A of sulphuretted hydrogen gas = 1·177, and the specific gravity B of hydrogen gas = 0·073. Hence A − B = 1·177 − 0·073 = 1·104 = specific gravity of gaseous sulphur.

Let us now derive the specific gravity of gaseous sulphur from that of sulphurous acid gas. The specific gravity of sulphurous acid gas is according to Thomson = 2·193, and that of oxygen gas = 1·103. Hence gaseous sulphur = 2·193 − 1·103 = 1·090, a number which differs from the former by only $\frac{1}{110}$. I, therefore, assume the round number 1·100 as the specific gravity of gaseous sulphur.

Sect. 29.—The specific gravity of carbonic acid is = 1·519, If from this we subtract 1·103 = specific gravity of oxygen gas, we get 0·416 = specific gravity of carbon in the state of gas. I take it for granted that it is known that when oxygen gas is converted into carbonic acid gas by the combustion of charcoal in it, the bulk is not altered.

We obtain almost the same number when we employ the specific gravity and the constituents of olefiant gas for the data of our calculation. This gas requires for complete combustion three times its volume of oxygen gas, and forms twice its volume of carbonic acid gas. Hence it follows that one volume of olefiant gas must be composed of two volumes of gaseous carbon and two volumes of hydrogen gas condensed into one volume. In this case, we have $a = 2$, $b = 2$. Now the specific gravity of olefiant gas, according to Thomson, is 0·974 = A, and the specific gravity of hydrogen gas 0·073 = B. Therefore the specific gravity of a volume of carbon in the state of gas $= x = \frac{0{\cdot}974 - \overline{2 \times 0{\cdot}073}}{2} = \frac{0{\cdot}828}{2} = 0{\cdot}414$; a number which does not deviate $\frac{1}{300}$th part from that found before. M. de Saussure has more lately found the specific gravity of olefiant gas somewhat heavier than the number given by Thomson. On that account I retain the number 0·416 for a volume of carbon in the following stochiometrical construction.

Sect. 30.—Now to exhibit our analysis according to the theory of volumes, it is merely necessary that the number of volumes of each constituent assumed correspond with the three data specified in sect. 22 and sect. 23; namely, the ratio of the sulphur to the carbon = 2·6 : 0·328. 2. The proportion of azotic gas to the hydrogen gas in volume = 1 : 3. 3. The quantity of sulphur in 100 parts of anthrazothionic acid = 67·3. Now these conditions are completely fulfilled when we state the elements of anthrazothionic acid in the following way in volumes. It consists of

$$\left.\begin{array}{l} 3 \text{ volumes hydrogen} = \\ \quad 3 \times 0{\cdot}073 \ \ldots\ldots = 0{\cdot}219 \\ 1 \text{ volume azote} \ldots\ldots = 0{\cdot}969 \end{array}\right\} = \begin{array}{l} 2 \text{ volumes ammonia} \\ = 2 \times 0{\cdot}594 \ldots\ldots = 1{\cdot}188 \end{array}$$

$$\overline{1{\cdot}188}$$

$$\left.\begin{array}{l} 1 \text{ volume carbon} \ldots\ldots = 0{\cdot}416 \\ 3 \text{ volumes sulphur}^{*} \\ \quad = 3 \times 1{\cdot}100 \ldots\ldots = 3{\cdot}300 \end{array}\right\} = \begin{array}{l} 1 \text{ or } 2 \text{ volumes sul-} \\ \text{phuretted carbon} \ldots = 3{\cdot}716 \end{array}$$

$$\overline{3{\cdot}716}\dagger \qquad \qquad \overline{4{\cdot}904}$$

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* Vauquelin found that Lampadius's sulphuret of carbon was a compound of 14 carbon and 86 sulphur. We may, therefore, in a stochiometrical point of view, consider it as a compound of one volume carbon and three volumes sulphur. This, when converted into weights, gives us 11·2 carbon and 88·8 sulphur, numbers which do not differ very far from Vauquelin's results.

† We may assume that one volume or two volumes of sulphuret of carbon combine chemically with two volumes of ammonia, and form anthrazothionic acid; but the accurate number of volumes cannot be determined, till the specific gravity of the imaginary sulphuretted carbon gas, at the temperature of 32°, and under a pressure of 30 inches of mercury, be accurately ascertained. Gay-Lussac has indeed determined the sp. gr. of sulphuretted carbon gas by experiments at the boiling water temperature to be 2·670. But it is a question whether this determination will apply to that of the imaginary sulphuretted carbon gas at the temperature of 32°? Gay-Lussac finds the specific gravity of vapour of alcohol = 1·500. But when we reduce the constituents of alcohol, as found by Saussure, to volumes, they must be four volumes of carbon = 1·664, one volume oxygen = 1·103, and six volumes hydrogen = 0·438. The sum of the weights of these volumes is = 3·205. Were we now to assume 1·500, the specific gravity of vapour of alcohol found by Gay-Lussac, as the true specific gravity of the imaginary vapour of alcohol at the freezing temperature, and determine from that the change of volume which the 11 volumes undergo, it is obvious that we could not obtain a whole

It is much to be wished that those chemists who are so fortunate as to have good apparatus at their disposal would ascertain accurately the specific gravity of gaseous anthrazothionic acid freed from water. This would enable us to determine the condensation which the proximate constituents of this acid (sulphuret of carbon and ammonia) undergo when they unite. If one volume of sulphuret of carbon and two volumes of ammonia were to constitute three volumes of anthrazothionic acid; that is to say, if the proximate constituents of the acid were to undergo no condensation when they unite, three volumes of the acid would weigh 4·904; and, of course, the weight of one volume would be 1·634.

If we reckon the constituents of 100 parts of the acid from the weight of the volumes thus found to constitute anthrazothionic acid, we shall obtain the very same results (a trifling variation in the decimals excepted) as those already obtained in sect. 24. But these last estimates I consider as most correct. We may, therefore, reckon 100 parts of anthrazothionic acid to be composed of

Sulphur.................	67·29	in weight
Carbon.................	8·48	
Azote...................	19·76	
Hydrogen..............	4·47	
	100·00	

Sect. 31.—This, as far as is known, is the only example of an acid containing an alkali, or, at least, its elements in the requisite proportions, as a proximate constituent.

Future experiments are requisite to inform us whether some other acids, as uric acid, sebacic acid, amniotic acid, &c. when stochiometrically analyzed, will not oblige inquiring chemists to draw the same conclusion with respect to them. Uric acid, at least, when treated with chlorine, always forms muriate of ammonia. Hence it is not improbable that in this acid ammonia exists converted into an acid in the same way by means of carbon, as it is in anthrazothionic acid by means of sulphur.

Berzelius has called those bodies *acids* which are attracted to the positive pole, and those *alkalies* which are attracted to the negative pole of the galvanic circle. But it is easy to see that no *acid* can be given which will not be *electropositive* with regard to

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number. We must divide the 11 not by 5, but by 5·14, which would give us 2·136 volumes; for it is obvious that if one volume weigh 1·500, 2·136 volumes would weigh 3·205. But the assumption of a condensation amounting to 5·14 does not agree with the observations hitherto made, that the condensation is always by whole numbers of volumes. Hence it follows that Gay-Lussac's estimates of the sp. gr. of alcohol vapour, sulphuret of carbon vapour, &c. cannot be taken as the sp. gr. of the imaginary vapours at the freezing point. Perhaps the sp. gr. of these last might be obtained, by saturating a gas of known sp. gr. with the vapours at the freezing point, and then from the weight of the mixture subtracting the known weight of the gas.—(See Haüy's Traité de Physique, i. 181. Second edition.)

a stronger acid; and no *base* which will not be *electronegative* with regard to a more powerful base. When, therefore, such compounds are exposed to the action of galvanism, the weaker acid of a double acid must pass to the negative pole, and the weaker base of a double base to the positive pole; so that the weaker acid will assume the character of a base, and the weaker base of an acid. Azote, iodine, and sulphur, sometimes put on the character of acids, sometimes of bases. According to this view of the subject, there can be no fixed acid with respect to all electronegative bodies, except oxygen; and no fixed base with respect to all electropositive bodies, except hydrogen. But oxygen is not *acid;* neither has hydrogen the properties of a base, or an alkali; so that this view of the subject obliges us to consider a substance as an absolute acid, which is not acid at all, and another as an absolute base, or an absolute alkali, which is not alkaline at all. On the other side it obliges us to reckon bodies which possess the distinguishing characters of *acids* and *alkalies;* namely, an acid and alkaline taste, the property of giving a red or a green colour to vegetable blues, &c. as neither acids nor alkalies. To make the terms *acid* and *electronegative, alkali* and *electropositive,* synonymous, is, in fact, to confound what ought to be separated. These anomalies, I conceive, I have cleared up in my observations on the definitions of *acid* and *alkali,* which were published four years ago.—(Schweigger's Journal, ix. 331.) In my opinion *an acid is a body, which, when dissolved in water, acts upon the liquid like the positive pole of a battery; while an alkali is a body which, being dissolved in water, acts upon it like the negative pole of a battery.* According to this explanation, we are not obliged to consider azote, sulphur, iodine, either relatively to acids or alkalies; for they may in certain compounds enter into electrochemical action, sometimes with the positive, and sometimes with the negative pole of the battery, just as these poles do with water; but these substances enter into no such action with the water, but seem to be quite *indifferent* with respect to it. Water at the positive pole shows, as is known, all the properties of an acid; it reddens vegetable blues, oxidizes metals, prevents (neutralizes) the action of alkalies. At the negative pole, on the other hand, it shows all the properties of an alkali; it precipitates the bases dissolved in acids; gives a green colour to vegetable blues; prevents (neutralizes) the action of acids. It has been ascertained besides, that these actions can continue in pure water only as long as the electrochemical action of the battery continues. From this we may in some measure comprehend how an alkali (ammonia), or its elements, by its union with another body (sulphuret of carbon), may alter its neutral electrochemical point so far as to assume all the properties of an acid: as a metal, for example, mercury, is capable, by uniting with even a very small quantity of another metal, of altering its electrical neutral point enormously.—(Ritter's System.) *

Sect. 32.—The number which belongs to anthrazothionic acid, in Wollaston's stochiometrical scale, will be obtained by determining the quantity of acid, which contains as much hydrogen

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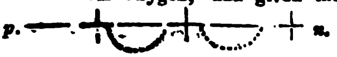
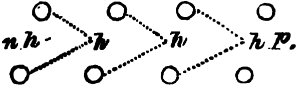
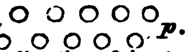
* I must notice, by the bye, that several electro-chemical statements of mine have been adopted by celebrated men, and even employed as the foundation of whole systems, without mentioning me as the original broacher of these views. Thus, for example, no one surely before me ascribed the light which is evolved during combustion to the union of the positive and negative electricity of the bodies acting upon each other. This I did in 1807.—(See Ann. de Chim. lxiii. 34.) I pointed out in the same paper, p. 24, why the electricity set free by chemical action is not capable of acting sensibly on the electrometer. The galvanic decomposition of water, a desperate problem, which Monge, Berthollet, Davy, Berzelius, have endeavoured in vain to explain (See Haüy's Traité de Physique, ii. 51; and Essai de Stat. Chim. i. 216), I completely explained towards the end of 1805, and founded on it a theory, which is so much supported by all analogous galvanical phenomena, that since that time it has been almost generally adopted. It must, therefore, appear surprizing to me that Berzelius, when in his excellent book entitled "Elements of Chemistry," he employs my theory exactly for the explanation of the galvanic decomposition of water, never so much as mentions the author of that theory; though he does so in every other similar case. In my first essay, I have assumed that water is a compound of one atom hydrogen and one atom oxygen, and given the following figure by way of illustration. 

In my second essay, I presumed that water might be considered likewise as composed of two atoms oxygen and one atom hydrogen, and gave the following figure in elucidation of the notion. 

The figure which Berzelius gives, differs but little from both these; namely, 

But it is easy to see that this last is inaccurate; for as the direction of the stream is from *n* to *p*, or the opposite way, and as water consists of only two atoms, the polarity of the elements of the water, which occasions the exchange, cannot be placed in rows perpendicular to each other as thus, *n* ± ± ± ± *p*; but as in my first figure, *p* — + — + — + *n*. In other respects, the fundamental idea is the same; namely, the simultaneous exchange of the elements of the water with the decomposition. Biot in his Traité de Physique, ii. 506, while accounting for the galvanic decomposition of water, neither mentions me nor any one else; but he gives exactly my theory, and says, "Il ne s'est elevé à cet égard qu'une opinion, qui ait soutenu les regards de l'experience." Favourable as this statement is for my theory, many a person, not much conversant with chemical literature, may be induced to believe from it that the opinion (as Biot terms it) has sprung up at once in the mind of Biot, and all other chemists, and that no one knows who first advanced it. I find myself, therefore, under the necessity of appealing to the public in this note. There is surely a great lack of chemical literature when a person expresses himself so indefinitely as Biot does in the passage quoted. (See likewise Thenard's Traité de Chimie, i. 104; and Klaproth and Wolf's Dictionary, first supplement, p. 692.)

It deserves attention, that I was very near Dalton's discovery, respecting the weight of atoms, while I was employed about the galvanic decomposition of water. The 37th and 38th figure in Dalton's System, vol. ii. plate 1, are quite the same as those that I gave long before in the Annales de Chimie (*loco citato*). But I acknowledge at the same time that the magnificent and bold idea of determining the weights of the atoms from the relative weights of the constituents of bodies did not occur to me.

as will saturate 10 of oxygen. Now 10 oxygen require 1·327 of hydrogen, and in 49·04 anthrazothionic acid there are 2·19 of hydrogen. Hence 1·327 hydrogen must be contained in 29·71 of acid. The number 29·71, therefore, is the equivalent for anthrazothionic acid; and if from this number we subtract 1·327, or the hydrogen, the remainder 28·39 will be the equivalent for anthrazothion. Future experiments must determine whether anthrazothionic acid will be formed when Lampadius's sulphuret of carbon and ammoniacal gas are made to pass over red hot potash.

ARTICLE III.

On the Sulphuretted Chyazic Acid of Porrett.[*] By M. Vogel.

When Mr. Porrett, an English chemist, was occupied in 1808, with examining the reciprocal action of prussian blue and sulphuret of potash, he discovered sulphuretted chyazic acid

The only set of experiments which has since appeared on this subject is contained in a memoir of M. Grotthuss of Courland.[†] This chemist made a great number of experiments on this acid and its salts, from which he has drawn as a conclusion that Porrett's acid is not composed of sulphur and prussic acid; but rather of the elements of that acid united to sulphur in very different proportions.

Formation of Sulphuretted Chyazic Acid.

Mr. Porrett has pointed out different methods more or less complicated of obtaining this acid. The method of M. Grotthuss seeming to me to present advantages, I repeated it by calcining in a covered crucible a mixture of two parts of prussiate of potash and one part of sulphur.

The black mass remaining in the crucible being boiled with alcohol of 38 degrees, gave a liquid, colourless after being filtered, which did not form prussian blue with the ferruginous salt, but communicated to them a dark cherry red colour.

The alcoholic solution, however, was very alkaline. Muriatic acid disengaged from it sulphuretted hydrogen gas, and acetate of lead occasioned a black precipitate. Hence it contained potash partly disengaged and partly combined with sulphuretted hydrogen. By M. Grotthuss's method then, we cannot obtain a pure sulphuretted chyazate, nor of consequence pure sulphuretted chyazic acid; for when the salt is mixed with diluted sulphuric acid and distilled, the acid which passes into the retort is contaminated with sulphuretted hydrogen.

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[*] Translated from the Journ. de Pharm. Oct. 1818, p. 441.

[†] A translation of this paper has just appeared in the *Annals of Philosophy*.

The reason why M. Grotthuss's process is unsatisfactory is, that the mixture of prussiate of potash and sulphur is exposed to too high a temperature; for I have ascertained that whenever this mixture is exposed to a red heat, or to the heat of a forge, as Grotthuss prescribes, potash is disengaged, and sulphuretted hydrogen formed. I have, therefore, varied the experiment, and have found that a pure sulphuretted chyazate may be formed by modifying the heat a good deal.

For this purpose, a mixture of equal parts of prussiate of potash and flowers of sulphur is put into a glass matrass, which is exposed to heat. After the matter has ceased to emit air bubbles, it is left for an hour in a state of fusion; but at a temperature greatly below a red heat. The matrass is then allowed to cool, it is broken in pieces, the black matter is reduced to powder, and hot water is poured upon it.* The filtered liquor is colourless, perfectly neutral, and contains no sulphuretted hydrogen. It is not altered when mixed with protosulphate of iron, and becomes red when mixed with persulphate of iron. If the filtered, colourless solution be left exposed for some time to the open air, or if it be placed in contact with nitrous acid in vapour, it becomes dark red, because it contains protoxide of iron combined with sulphuretted chyazic acid and potash. The newly prepared liquid, when mixed with ammonia, allows the green hydrate of iron to precipitate; while prussiate of potash throws down prussian blue.

When the object in view is to obtain sulphuretted chyazic acid, this oxide of iron occasions no obstacle. But if we wish to get a pure sulphuretted chyazate, we must drop into the solution caustic potash till the whole iron is precipitated. The liquid is then filtered while still hot, and evaporated to dryness. The salt obtained is very soluble in alcohol. It does not become red when exposed to the air; but it is very deliquescent, and ought, therefore, to be immediately put into a well stopped bottle. The process for obtaining pure sulphuretted chyazate of potash may, therefore, be reduced to this:

Keep a mixture of equal weights of prussiate of potash in powder and flowers of sulphur in fusion in a matrass for an hour. When the mass has cooled and been reduced to powder, treat it with twice its weight of distilled water, and into the distilled solution drop potash as long as any precipitate falls. Filter a second time, and evaporate the liquid to dryness.

Extraction of Sulphuretted Chyazic Acid.

After having obtained a *pure* sulphuretted chyazate, it is possible to procure likewise a *pure* sulphuretted chyazic acid.

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* To determine whether the mass has been kept a sufficient time in a state of fusion, and whether all the common prussiate of potash has been decomposed, dissolve a little of it in water, and try the solution with sulphate of iron. If no prussian blue is formed, the decomposition is complete. If it is, we must treat the powder with boiling alcohol, or fuse it a second time.

For this purpose I dissolved an ounce of sulphuretted chyazate of potash in an ounce of water; I poured the solution into a tubulated retort, and added six gros of concentrated sulphuric acid, previously diluted with its own weight of water. The retort having a receiver fitted to it was placed upon a sand bath, and heated.

The product of this distillation is a limpid, colourless liquid. What comes over first contains more water than the succeeding portions. It becomes more and more acid as the process advances. The distillation may be continued as long as the liquid passes over colourless; and this colourless liquid only should be regarded as pure sulphuretted chyazic acid. It ought to be preserved in small bottles, which should be quite filled with it. The liquid which comes over last is yellow, and contains a little sulphur in solution, and sometimes even hydrosulphuret of ammonia. It ought not to be added to the colourless acid.

There remains in the crucible, besides sulphate of potash, a powder of a deep orange colour, containing sulphur and charcoal.*

Properties of pure Sulphuretted Chyazic Acid.

When most concentrated, it is a colourless liquid with a pungent smell, reddening vegetable blues, and having an acid taste. Its specific gravity at the temperature of 61½° is 1·0203.

To satisfy myself whether my sulphuretted chyazic acid contained any prussic acid, I super-saturated it with potash, and added to the newly prepared solution protosulphate of iron. But not the least prussian blue was formed, even when an acid was added to the liquid.

Neither does it contain any sulphuric or sulphurous acid; for it is not precipitated by barytes water. The white crystalline precipitate, produced by acetate of lead, is entirely soluble in cold water. This last experiment proves likewise that the acid contains no sulphuretted hydrogen.

Action of Heat on Sulphuretted Chyazic Acid.

The acid requires, when the barometer stands at 28·24 inches, a temperature of 216½°, to cause it to boil.

When acid is boiled in a glass filled with mercury, the column of mercury sinks completely, and recovers its former height when the acid is allowed to cool. Hence the acid, when boiled, is converted into vapour, but not into gas.

When the acid is poured into a red hot platinum crucible, sulphur is disengaged, and at last burns with a blue flame.

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* I have likewise distilled sulphuretted chyazate of potash with an addition of phosphoric acid, and have obtained very pure sulphuretted chyazic acid. But no orange powder remains in the retort; and it appears that sulphuretted chyazic acid is not decomposed by phosphoric acid.

I passed the vapour of the acid through a red hot porcelain tube, and obtained at the furthest extremity of it, sulphur, sulphuretted chyazic acid undecomposed, and prussic acid, which was partly saturated with ammonia. But I found no charcoal in the tube; though I have no doubt that if the acid be passed very slowly through a red hot tube, it will be completely decomposed, and will deposit charcoal.

When the acid is passed through a red hot porcelain tube filled with iron turnings, we obtain sulphuret of iron, prussic acid, and sulphuretted hydrogen.

Sulphuretted chyazic acid crystallizes in six-sided prisms at the temperature of 54½°.

Action of Air.

When pure concentrated acid is exposed to the air in an open vessel, it soon begins to evaporate. A slip of paper, with which the mouth of the vessel was covered, became red; while the acid assumed a yellow colour, and deposited sulphur.*

Mr. Porrett states that the acid becomes red on the contact of air, and that it is oxydized; but I cannot adopt his opinion. The red colour doubtless proceeded from some protoxide of iron which had passed into peroxide by the contact of the air, or it might, perhaps, have been occasioned by the contact of paper, or other organic bodies floating about in the air. The sensibility of this acid for peroxide of iron is so great that it cannot be filtered through paper, or placed in contact with cork, without becoming red.

Action of Chlorine.

When sulphuretted chyazic acid was mixed with liquid chlorine, this last substance lost its odour and its yellow colour. The mixture was now precipitated by muriate of barytes, which was not the case before. Hence sulphuric acid had been formed; but no sulphur was deposited.

Neither does chlorine precipitate sulphur from the sulphuretted chyazate of potash; though M. Grotthuss has founded a mode of analyzing this acid upon the separation of the sulphur in this case. I have already shown that the sulphuretted chyazate of potash, procured by Grotthuss, by means of a red heat, contained sulphuretted hydrogen. This was the reason why chlorine precipitated sulphur from it.

Sulphuretted chyazic acid is completely decomposed, when agitated with chlorine; for the mixed liquid, after being saturated with potash, is no longer reddened by persulphate of iron. But prussian blue is formed, which is insoluble in muriatic acid.

When a mixture of sulphuretted chyazic acid and chlorine is slightly heated, a very distinct odour of prussic acid becomes perceptible. If this vapour is made to pass into lime water, we obtain prussiate of lime, which forms prussian blue with ferruginous solutions.

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* When exposed in small bottles to the rays of the sun, it becomes likewise yellow, and deposits sulphur.

When sulphuretted chyazic acid is poured into a vessel filled with chlorine gas, the temperature rises considerably. The whole of the sulphur is converted into sulphuric acid, and of course no sulphur is precipitated.

Thus it appears that the action of chlorine upon sulphuretted chyazic acid consists in converting the sulphur into sulphuric acid, and setting the prussic acid at liberty. It is necessary to employ a slight excess of chlorine in order to acidify the whole of the sulphur; but if the proportion used be too great, it would combine with the prussic acid when set at liberty.

The opinion of Mr. Porrett, that sulphuretted chyazic acid is a compound of prussic acid and sulphur, is not so absurd as M. Grotthuss conceives it to be; for can it be said that the chlorine formed prussic acid? This would be a thing without example in chemical science.

The sudden death which sulphuretted chyazic acid produces in animals seems still further to favour the notion, that it is not merely the elements, but the prussic acid itself, which acts. It is a most striking fact that prussic acid, when it combines with sulphur, loses its most remarkable properties.

Nitric or nitrous acid does not precipitate sulphur from sulphuretted chyazic acid. All the sulphur is converted into sulphuric acid, and the prussic acid becomes free. Concentrated sulphuric acid is the only acid which precipitates sulphur from sulphuretted chyazic acid.*

Action of Iodine.

When sulphuretted chyazic acid was boiled with iodine, there passed into the receiver, which contained lime water, a quantity of prussic acid. The liquid which remained in the crucible had a reddish brown colour, and was very acid, but did not contain prussic acid. When this liquid was neutralized by ammonia, it became colourless. It was precipitated *red* by corrosive sublimate; orange *yellow* by acetate of lead; and *green* by protonitrate of mercury. The sulphuretted chyazic acid of course had been decomposed, and hydriodic acid formed.

On an analogous Property between Sulphuretted Chyazic Acid and Meconic Acid.

Sulphuretted chyazic acid, when mixed with ferruginous salts, produces exactly the same appearances as meconic acid. Neither of these acids occasions any change of colour in the protosalts; but both of them dissolve the peroxide of iron, and form with it a blood red solution; and both of them give the same colour to the persalts of iron.

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* When I placed sulphuretted chyazic acid in the circuit of a Voltaic battery of 50 pair of plates, I observed at the negative pole a considerable disengagement of gas, while sulphur was deposited round the positive pole.

The red solutions, produced by both of these acids, lose their colour on the addition of acids, alkalies, protomuriate of tin, and the solar rays.

The disappearing of the red colour by the sun's rays is owing to the peroxide of iron being changed into protoxide; for ammonia throws down a reddish precipitate from the red solution; but a green precipitate (protohydrate of iron) from the liquid rendered colourless by the solar light.

When the liquids rendered colourless by the sun are placed in a dark place, but exposed to the air; or better, if they be placed in contact with nitrous acid in the state of vapour, the blood red colour appears again; because the iron is again converted into peroxide.

Writing with common ink becomes red, when plunged into sulphuretted chyazic acid, as it does when plunged into meconic acid.

But the analogy between these two acids does not go further.

Solution of gold deprives the compound of sulphuretted chyazic acid and peroxide of iron of its red colour; but it produces no alteration in the solution of peroxide of iron in meconic acid.

Finally, meconic acid is solid, crystallizable, and capable of subliming, while sulphuretted chyazic acid is a liquid, and a violent poison.

Dr. Sœmmering made a set of experiments on dogs with sulphuretted chyazic acid, meconic acid, and morphia. We cannot enter into a detail of the numerous experiments which he made on this subject. As a paper on the subject will be published in Schweigger's Journal, we shall confine ourselves here to the following observations which contain the general results.

Outline of the Physiological Experiments.

Concentrated chyazic acid occasions sudden death, when administered in the quantity of half a gros. When the acid is much diluted with water, and given in repeated doses, it acts on the organs of respiration, produces convulsions, and death ensues more slowly.

A small quantity of this acid affects the respiration. The acid is voided in the urine, without producing permanently bad consequences.

A dog, upon which the diluted acid was made to act for 24 hours, and which died in consequence, was opened. The presence of the acid could be detected in the blood, and still more easily in the urine.

Sulphuretted chyazate of potash, administered in the same doses, produces similar effects. This salt then, as well as the acid, acts in the same way as prussic acid.

Meconic acid, taken in a dose of from eight to ten grains, produces no sensible effect upon young and weak dogs: and the original opinion of M. Sertürner seems to be much better founded than the recent assertion *that meconic acid is the most violent poison among vegetable substances.*

Meconiate of soda, in a dose of 10 grains, produces no sensible effect.

Morphia, in a dose of 10 grains, or even four grains, is narcotic in an eminent degree. A dog fell asleep immediately, and slept 24 hours without interruption; but did not die.

Recapitulation of the Chemical Experiments.

It follows from the facts stated in this paper:

1. That we cannot obtain pure sulphuretted chyazate of potash, nor pure sulphuretted chyazic acid, when we calcine prussiate of potash and sulphur at a red heat.
2. That it is sufficient to fuse the mixture, if we do not wish to push the decomposition further than is necessary for the purity of the products.
3. That we may obtain pure sulphuretted chyazic acid by distilling sulphuretted chyazate of potash, mixed with dilute sulphuric acid, or still better with phosphoric acid.
4. That sulphuretted chyazic acid exposed to the sun, or placed in contact with the air, allows sulphur to precipitate without assuming a red colour.
5. That the acid, when exposed to a red heat, is decomposed into sulphur, prussic acid, and ammonia.
6. That nitric acid, or chlorine, does not precipitate sulphur from sulphuretted chyazic acid, but forms sulphuric acid, and sets prussic acid at liberty.
7. That iodine decomposes the acid, and produces hydriodic acid.
8. That sulphuretted chyazic acid has no other analogy with meconic acid than that of forming blood red liquids with peroxide of iron and the persalts of iron.
9. That sulphuretted chyazic acid is an excellent reagent for salts containing peroxide of iron; but only when there is no excess either of acid or alkali in the liquid.
10. That sulphuretted chyazic acid is not composed of the elements of prussic acid in other proportions united to sulphur; but appears to consist of a chemical combination of prussic acid and sulphur; and the sulphur is the cause of all the singular properties of this acid composed of three combustible bodies.
11. Finally, that the discovery of Porrett should make us attentive in manufactures of prussian blue to avoid a potash which contains sulphur, or even too great a quantity of sulphate; because it would occasion a considerable loss in the formation of the prussian blue.

Article IV.

Account of a newly discovered Metal, and the Analysis of a new Mineral. By Prof. Stromeyer. (In a Letter to Dr. Schweigger.) *

Gottingen, April 26, 1818.

The last number of your excellent journal, which I received yesterday, and which, among other interesting discoveries and researches, gives an account of a new metal discovered by Berzelius, has suggested to me the propriety of sending you for the same publication an account of a new metal discovered by me during the course of the last winter.

As I was last harvest inspecting the apothecaries' shops in the principality of Hildesheim, in consequence of the general inspection of the apothecaries of the kingdom having been entrusted to me by our most gracious Regency, I observed in several of them, instead of the proper oxide of zinc, carbonate of zinc, which had been almost entirely procured from the chemical manufactory at Salzgitter. This carbonate of zinc had a dazzling white colour; but when heated to redness, it assumed a yellow colour, inclining to orange, though no sensible portion of iron or lead could be detected in it. When I afterwards visited Salzgitter, during the course of this journey, and went to the chemical manufactory from which the carbonate of zinc had been procured; and when I expressed my surprize that carbonate of zinc should be sold instead of oxide of zinc, Mr. Jost, who has the charge of the pharmaceutical department of this manufactory, informed me that the reason was, that their carbonate of zinc, when exposed to a red heat, always assumed a yellow colour, and was on that account supposed to contain iron, though the greatest care had been taken beforehand to free the zinc from iron, and though it was impossible to detect any iron in the oxide of zinc itself. This information induced me to examine this oxide of zinc more carefully, and I found, to my great surprize, that the colour which it assumed was owing to the presence of a peculiar metallic oxide, the existence of which had not hitherto been suspected. I succeeded by a peculiar process in freeing it from oxide of zinc, and in reducing it to the metallic state. I have found the same oxide in *tutia*, and in several other oxides of zinc; and it exists likewise, as might have been expected, in metallic zinc. But in all these bodies it exists only in a very minute proportion, which can scarcely exceed between $\frac{1}{1000}$ and $\frac{1}{100}$ of the whole.

The properties by which this new metal is distinguished are the following: it has a light white colour, inclining a little to grey, and in this respect comes nearest to platinum. It has a great deal of brilliancy, and admits of a fine polish. Its texture s very compact, and its fracture hackly. Its specific gravity is pretty considerable, amounting to 8·750 after the metal has been fused. It is very ductile, and may be hammered out into thin plates, both cold and hot, without the risk of cracking. Its cohesion appears to be pretty considerable, and to surpass that of tin. It belongs to the more fusible metals; for it melts before it is red hot; and an iron wire, heated to redness by a spirit lamp, readily melts it. It is likewise very volatile, rising up in the state of vapour, at a temperature not much surpassing that at which mercury boils. This vapour has no peculiar smell, and congeals, like mercury, in drops, which exhibit distinct traces of crystallization.

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* Translated from Schweigger's Journal, xxi. 297. (Published May 28, 1818.)

This metal undergoes no alteration when exposed to the air; but, when heated, it burns very readily, and is converted into a yellow coloured oxide, the greater part of which sublimes in the state of a yellowish coloured smoke, and covers any body held over it with a yellow coating. If the experiment be made before the blow-pipe upon charcoal, the charcoal is in like manner covered with a brownish yellow-coloured coat. It gives out no perceptible smell when it burns. It dissolves in nitric acid with the evolution of nitrous gas. Sulphuric acid and muriatic acid act upon it likewise, and hydrogen gas is given out; but its solution in these acids is a very slow process. The solutions are quite colourless, and are not precipitated by water. This metal appears to combine with oxygen in only one proportion. The oxide has a greenish yellow colour; but by exposure to a strong red heat, it acquires a tint of yellow; and if the heat be very long continued, it becomes nearly brown. As the orange and brown oxides dissolve in acids, as well as the greenish yellow, without the evolution of any gas, and form the very same kind of solutions, there is reason to believe that the alteration in the colour of the oxide is merely owing to the state of its aggregation, and not to any difference in the proportion of oxygen which it contains. This oxide withstands the strongest heat; and when raised to a white heat in a covered platinum crucible, by means of Marcet's lamp, it did not undergo fusion. When heated with charcoal, or any substance containing carbon, it is easily reduced to the metallic state; and the reduction takes place when the heat just begins to get red. To borax, it communicates no colour. It does not dissolve in the fixed alkalies; but a portion of it is taken up by ammonia. Towards the acids, it acts precisely as a salifiable base. The salts which it forms have almost all a white colour. Those with sulphuric acid, nitric acid, muriatic acid, and acetic acid, crystallize readily, and are very soluble. Those with phosphoric acid, carbonic acid, and oxalic acid, are insoluble. From the solutions of the first mentioned salts, it is thrown down white by the fixed alkalies, probably in the state of an hydrate, and the precipitate is not redissolved by adding an excess of alkali. By ammonia, on the contrary, it is indeed at first precipitated white; but when an excess of the ammonia is added, it is again taken up. By carbonate of ammonia, it is thrown down in the state of a carbonate; but when an additional quantity of the precipitant is added, the greatest part of this carbonate is again redissolved. When this solution is exposed to the open air, the carbonate very speedily precipitates again. We may, therefore, employ carbonate of ammonia with advantage to separate this metal from zinc and copper, when it is mixed with them.

Prussiate of potash throws down this metal from its solutions in acids *white;* sulphuretted hydrogen, and the hydrosulphurets, throw it down yellow. This last precipitate, which, when dried, has an orange yellow colour, resembles *sulphur auratum,* and like it is a hydrosulphuret. From its colour and appearance, it might, by a careless observer, be mistaken for orpiment; but it is readily distinguished by its more pulverulent form, by its appearance before the blow-pipe, and by its easy solubility in acids, with the evolution of sulphuretted hydrogen gas. To judge from some trials, this compound of sulphuretted hydrogen and the new metallic oxide is well adapted for painting, both with water and with oil. It forms a very good yellow, which is durable; and in point of beauty is not inferior to chrome yellow. This metal is precipitated by zinc from its acid solutions, reduced, and in the dendritic state. But copper, lead, silver, and gold, are precipitated by it in the metallic state.

The compounds which this metal forms with sulphur, phosphorus, iodine, and the other metals, I have not hitherto been able to investigate with accuracy; though it seems to unite readily with several of these bodies; for example, when heated with platinum, it easily melts, and combines with that metal, and it forms with mercury a solid crystallizable amalgam. Hitherto I have not been able to make it unite with copper.

These are the particulars which I have hitherto been able to ascertain respecting this metal. They are so peculiar that I entertain no doubt about it being a new metal quite different from every other. As I found it first in the oxide of zinc, I have taken occasion from that circumstance to give it the name of *Cadmium.*

In consequence of the very small quantity in which cadmium exists in the oxide of zinc, and the metallic zinc examined by me, it has not hitherto been in my power to undertake experiments to determine the composition of its compounds, the shape of the crystals of its salts, and the action of its oxide and salts on organized bodies, &c. Indeed the whole of the metal which I had for my experiments did not exceed three grammes. I am happy, therefore, to be able to inform you, that within these few days, through Mr. Hermann, of Schönebeck, and Dr. Rodolff, of Magdeburg, who took an interest in this metal, I have been placed in a situation which will enable me to carry my experiments further.

During the apothecary's visitation in the state of Magdeburg some years ago, there was found in the possession of several apothecaries, a preparation of zinc from Silesia, made in Hermann's manufactory at Schönebeck, which was confiscated on the supposition that it contained arsenic, because, when dissolved in acids, and mixed with sulphuretted hydrogen, it let fall a yellow precipitate, which, from the chemical experiments made on it, was considered as orpiment. This fact could not be indifferent to Mr. Hermann, as it affected the credit of his manufactory, and the more especially as the Medicinal Counsellor Roloff, who had assisted at the apothecaries' visitation, had drawn up a statement of the whole, and sent it to Hufeland, in Berlin, who published it in the February number of his Medical Journal. He, therefore, subjected the suspected oxide of zinc to a careful examination; but he could not succeed in detecting any arsenic in it. He then requested the Medicinal Counsellor Roloff to repeat his experiments on the oxide once more. This he did very readily. And he now perceived that the precipitate which had at first been taken by him for orpiment, was not so in reality; but owed its existence to the presence of another metal, having considerable resemblance to arsenic, but probably new. To obtain full certainty on the subject, both the gentlemen had recourse to me, and have sent me, within these few days, both a portion of the Silesian oxide of zinc and specimens of the orpiment, like precipitate, and of the metal extracted from it, with the request that I would subject these bodies to a new examination, and in particular that I should endeavour to ascertain whether they contained any arsenic.

From the particulars already stated, I considered it as probable that this Silesian oxide of zinc contained likewise the metal which I had discovered; and as it gives with sulphuretted hydrogen a precipitate similar in colour to orpiment, I considered this to be the reason why the oxide was supposed to contain arsenic. Some experiments made upon it fully confirmed this opinion. I have, therefore, informed Mr. Hermann of the circumstance by the post; and I shall not fail to give the same information to Medicinal Counsellor Roloff, whose letter I only received the day before yesterday.

As this Silesian oxide of zinc contains a much greater proportion of cadmium the oxide which I examined, amounting, according to the experiment of Hermann, to about three per cent. I hope now to have it in my power to procure a sufficient quantity of this metal to be able to examine it completely. I have, therefore, requested Mr. Hermann to send me an additional quantity of the oxide by the post; and I hope to receive it in the course of next week.

I shall conclude this letter by informing you of a new mineral, very remarkable, on account of its composition. I have given it the name of Polyhalite. According to my analysis, 100 parts of it contain the following ingredients:

Hydrous sulphate of lime.	28·74
Anhydrous sulphate of lime.	22·36
Sulphate of potash	27·48
Anhydrous sulphate of magnesia	20·11
Common salt.	0·19
Oxide of iron.	0·32
	99·20

This mineral occurs in the beds of rock salt at Ischel, in Upper Austria, and has been hitherto erroneously considered by mineralogists as *muriacite*; and under the name of *fibrous muriacite,* it has been described as a variety of that mineral substance.

Article V.

Researches on the Measure of Temperatures, and on the Laws of the Communication of Heat. By MM. Dulong and Petit.*

Introduction.

From the beginning of experimental physics, it has been perceived that of all the effects produced by heat, the changes of bulk which bodies undergo ought to be preferred to all the other phenomena due to the same cause to measure the natural or artificial changes of temperature. But there was a great distance between this first perception and the knowledge requisite to subject the construction of thermometers to invariable processes, which should render their indications comparable with each other. The frequent employment of these instruments, and the utility of the data which they furnish, have often drawn the attention of philosophers to all the circumstances that can contribute to their perfection. And all these circumstances have been studied with so much care, and at such length, that nothing further remains to be desired relative to that object.

Great precision was doubtless indispensable in thermometrical observations; but this was not sufficient to lead to an accurate knowledge of the theory of heat. We might indeed refer all the phenomena to an arbitrary scale of temperature, and form empirical formulas, which should represent the observations with precision. But we cannot hope to discover the most general properties; or, if the expression is preferred, the most simple laws of heat, till we have compared thermometers constructed with substances taken from the three general states in which matter exists, and till we have calculated the corresponding quantities of heat.

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* Translated from the Ann. de Chim. et Phys. vii. 113. This memoir gained the prize voted by the Academy of Sciences in the public meeting of March 16, 1818.

Though this subject of research must naturally have presented itself to the mind of every philosopher, we must acknowledge that it has not yet been treated in a manner suitable to its importance. The essays of Deluc and Crawford embrace too small a portion of the thermometric scale to enable us to deduce general consequences from them. Indeed this is a reproach which applies to almost all the experiments relative to the theory of heat; and it has become the source of a great number of erroneous inductions. Indeed it is easy to conceive that phenomena subjected to very different laws may appear identical within a certain interval of temperature, and that if we remain satisfied with observing them within those limits in which their divergence is almost insensible, we shall be led to ascribe their feeble discordance to errors of observation, and shall be destitute of the data requisite to mount to their real cause. We shall have occasion several times in the course of this memoir to show the justice of this reflection.

Mr. Dalton, considering this question from a point of view much more elevated, has endeavoured to establish general laws applicable to the measurement of all temperatures. These laws, it must be acknowledged, form an imposing whole, by their regularity and simplicity. Unfortunately this skilful philosopher proceeded with too much rapidity to generalize his very ingenious notions; but which depended upon uncertain data. The consequence is, that there is scarcely one of his assertions but what is contradicted by the result of the researches which we are now going to make known.

These researches have for their principal object the laws of the cooling of bodies, plunged into an elastic fluid of any nature whatever, and at different densities and temperatures. Before studying this class of phenomena, it was indispensable to obtain more exact ideas than we at present possess respecting the measure of elevated temperatures. It was by the examination of this accessory, but highly interesting question, that we began our labours. We shall likewise begin our memoir with it.

This memoir then will consist of two very distinct parts. The first will have for its object every thing which relates to the measure of temperature; the second will contain the general laws of cooling.

Part I.—*Of the Measure of Temperatures.*

If there existed a body whose dilatations were subjected to a law, so simple and so regular that successive additions of equal quantities of heat produced always the same increase of volume, such a body would possess all the requisites which philosophers have judged necessary and sufficient to constitute a perfect thermometer.

Such an instrument, however, might not offer all the advantages which it appears at first to promise. If the specific heat of all other bodies, for example, when referred to this thermometer, were variable, and unequally variable, in each of them, it is very evident that we could conclude nothing, *à priori*, from the indication of this thermometer relative to the quantities of heat acquired or lost by a determinate variation of temperature.

We see then that the first step to be taken in this research is to ascertain if the capacity of a great number of bodies, taken with the same scale, vary in the same manner; and if the dilatations of bodies, which differ most in their nature, are subjected to the same laws. This last comparison, with which we shall begin, being susceptible of a greater degree of precision than the first, we have extended it much further, and we think that we have taken every possible care to secure the accuracy of the results.

Of the Dilatation of the Gases.

When we have no other object but to establish a general comparison between the dilatations of all bodies, the thermometric substance to which all the measures are referred may be chosen in an arbitrary manner. The construction of the mercurial thermometer being easier, and its use more convenient, we have employed it in almost all our experiments.

The comparison of this thermometer with the air thermometer has been made long ago by Gay-Lussac, between the limits of freezing and boiling water. It results from the experiments of this celebrated philosopher that the two instruments do not present any sensible discordance within that interval of temperature.

Mr. Dalton thinks, on the contrary, that the mercurial thermometer would be about 1° higher than the air thermometer towards the middle of the scale, where the difference would obviously be the greatest, since the two instruments agree at 0° and at 100°.*

We see from this, that if there exist really a difference between the dilatabilities of air and mercury, it must be very small between the limits of freezing and boiling water.

We at first pursued this comparison for inferior temperatures. In a first experiment made at − 20°, we found a perfect identity between the two instruments; and by a great number of observations made from − 30° to − 36°, we observed slight differences; but sometimes positive, and sometimes negative, so that the mean of all the measures taken simultaneously on the two instruments is the same for each.* Thus in an extent of more than 130°, the difference of the two scales which we compare is sufficiently small to be confounded with the errors of the observations.†

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* Of the Centigrade Scale.

Nothing is easier than these kinds of experiments, as long as we do not go higher than the temperature of boiling water. But when we wish to prosecute this examination at higher temperatures, we experience great difficulties, depending partly upon our finding no longer any fixed temperature, and partly upon the great rapidity with which the liquid masses, in which the experiments are made, cool down. For this reason, and several others which it is useless to state, we are obliged to have recourse to more tedious and complicated processes. Those which we adopted, after studying carefully all the causes of error which are likely to occur, appear to us to attain the greatest possible precision in researches of this nature. We dispense, however, with giving a detailed description of them here, because they differ very little from those which we have already made known in a memoir inserted in the second volume of the Annales de Chimie et Physique, p. 240.‡ Our results,

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* In order to enable the reader to judge of the small deviations of the partial determinations, we shall state here some of those which were taken between − 30° and − 36°.

Mercurial thermometer.	Air thermometer corrected for the dilatation of the glass.
− 36·29°	− 36·18°
− 34·72	− 34·84
− 33·31	− 33·40
− 32·27	− 32·13
− 31·63	− 31·54
− 31·26	− 31·04
− 30·46	− 30·59
− 29·68	− 29·64
Mean − 32·452	Mean − 32·420

† The very considerable number of experiments which we shall have to state in this memoir does not permit us to enter into details relative to each of them. We shall, therefore, satisfy ourselves with giving the results obtained in each case, suppressing the intermediate calculations which led to them.

‡ Of all the means indicated in this memoir for measuring the dilatation of air, we shall only recapitulate the following, which has been most frequently employed.

The air is inclosed in a tube perfectly dried, placed horizontally in a bath of fixed oil, the temperature of which is gradually elevated. This tube terminates in the outside of the bath in a very fine tube, whose capacity is only a negligible fraction of the total volume. When the air has acquired the requisite temperature, the fine point of the tube is shut by means of a blow-pipe. It is then withdrawn from the bath, and when it has recovered the temperature of the air, the point is broken off under mercury. A portion of this fluid of course enters into the tube. By comparing the weight of it with that of the mercury which fills the whole tube, we can easily determine the dilatation of the air, taking into consideration always the difference of pressure.

This process requires only a slight change, when we operate on a gas different from air. The point of the tube must then be bent, and plunged into a capsule filled with mercury. While the temperature is increasing, a portion of the gas is driven out into the *external air; but when* the cooling begins, the mercury makes which may be seen in the following table, approach very nearly to those of the memoir above quoted, in the temperatures common to the two tables. But the following embraces almost the complete scale of mercury from the freezing to the boiling point of the liquid, which is an interval of about 400°.

TABLE I.

Temperature indicated by the mercurial thermometer.	Corresponding volumes of the same mass of air.	Temperature indicated by an air thermometer, corrected for the dilatation of the glass.
− 36°	0·8650	− 36·00°
0	1·0000	0·00
100	1·3750	100·00
150	1·5576	148·70
200	1·7389	197·05
250	1·9189	245·05
300	2·0976	292·70
360 { Boiling point of mercury. }	2·3125	350·00

The temperatures, indicated in the last column, have been corrected for the dilatation of the glass, which we shall immediately point out.

There exists a very great disagreement among the numbers given by different philosophers for the boiling point of mercury. This depends in part upon the greater or smaller care bestowed by each upon the construction of his instruments, and upon the accuracy of the correction which it is necessary to make for the portion of the tube which is not plunged in the liquid. The method which we have employed dispenses with this correction. Instead of measuring immediately the augmentation of volume of the same mass of mercury, as is done in the ordinary thermometers, we have determined the loss of weight which a mass of mercury, capable of filling a glass at zero, sustains when completely plunged into boiling mercury. Knowing the apparent dilatation of mercury in glass for the first 100°, we can, by a very simple calculation, find the corresponding temperature on a mercurial thermometer, whose tube is at the same temperature as the bulb. To prevent the liquid contained in the vase from boiling, the precaution was taken to make it terminate in a very narrow vertical tube, six centimetres in length. The liquid column, which it contained, did not make the 6000th part of the total mass; but by the pressure which it exercised in the interior of the vase, it completely prevented the formation of vapours. It is needless to say that great care was taken to expel every trace of air or humidity.

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its way by little and little into the tube till the bath has reached the temperature of the air. The calculation is then the same as in the case of air; and the only temperature to measure is the *maximum*, which may be obtained with the greatest precision.

The corresponding temperature of the air thermometer was calculated by a method analogous to that which we have constantly employed in our experiments on the dilatation of gases. The numbers given in the preceding table are the means of four results, which do not differ from each other a single degree.

Before going further, we shall make a remark of considerable importance. If we calculate the temperatures of an air thermometer by the augmentation of volume which the same mass of this fluid experiences under a constant pressure, we obtain exactly the same results as when we deduce them from the measure of the change of elasticity, the volume remaining the same. This result proves evidently that the law of Mariotte never ceases to be exact, whatever be the temperature.

From the beautiful observation of Gay-Lussac, that all elastic fluids undergo exactly the same dilatation from 0° to 100°, it was very probable that the same uniformity would be observed at high temperatures, and that the preceding numbers for air would apply to all gases. Yet that nothing might be left uncertain relating to a subject of such importance, we made an experiment on hydrogen gas, which, as is known, differs the most from the others in its physical properties. The result was included between the extremes of those which we had obtained for air.* We may, therefore, consider it as established that all the gases dilate absolutely in the same manner and the same quantity by equal changes of temperature.

The determinations which we have just stated would be sufficient, if it were required only to know the volume of a gas at any temperature whatever of the mercurial thermometer, or reciprocally; but the object which we had in view of comparing the respective dilatations of mercury and air is not yet completely attained. For all liquid thermometers indicate merely the difference of the expansion of the fluid, and of the vessel which contains them. But this difference cannot bear the same ratio with the absolute expansion of the liquid, excepting in the single case when the increments of volume of the two bodies follow exactly the same law. If, for example, the matter of the vessel dilated itself, according to a less rapid law than the liquid which it contains, it is evident that the thermometer would appear to rise even when the dilatation of the liquid was uniform. On the opposite supposition, there would take place a partial and unequal compensation, which would equally disturb the accuracy of the comparison. It was, therefore, indispensable to endeavour to ascertain the variation which the absolute dilatation of one of the two bodies constituting the mercurial thermometer experiences at elevated temperatures.

When we consider all the difficulties inherent in the measurement of the expansion of solids, even below the temperature of boiling water, we are terrified at the much more numerous obstacles which would accompany the same determination at elevated temperatures. After a careful consideration of all the experimental resources which we could hope for, the uncertainty of success and the enormous complication of apparatus that would have been required, determined us to undertake the direct measurement of the absolute dilatation of mercury. This is the object of the following chapter.

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* The volume of hydrogen being 1 at zero, we found it equal to 2·1003 at the temperature of 300°, of the mercurial thermometer. The extremes of the volume occupied by air, in the same circumstances, are 2·0948 and 2·1027.

Of the Absolute Dilatation of Mercury.

The knowledge of the absolute dilatation of mercury became essential as soon as it was perceived that heights might be exactly measured by means of the barometer. Nor is the datum less useful in many physical experiments. Accordingly, there are few determinations which have given rise to so many researches. But notwithstanding all the precautions of experimenters to obtain accuracy, there are few examples of greater discordance than are to be seen in the results which they have obtained. The following are some of them.

Absolute Dilatations of Mercury.

Dalton	$\frac{1}{50}$	Lavoisier and Laplace	$\frac{1}{55}$
Lord Charles Cavendish	$\frac{1}{53}$	Haellström	$\frac{1}{55}$
Deluc	$\frac{1}{56}$	Lalande and Delisle	$\frac{1}{66}$
General Roy	$\frac{1}{59}$	Casbois	$\frac{1}{67}$
Shuckburgh	$\frac{1}{54}$		

The greater number of these determinations have been calculated by adding to the apparent dilatation of mercury in glass the proper dilatation of this last substance. And as we were long uncertain of the true expansion of glass, the preceding results behoved to share this uncertainty.

Deluc, Casbois, and General Roy, endeavoured to measure directly the real dilatation of mercury by the increase of the barometrical column, occasioned by a known variation of temperature. The results obtained in this way are very inexact. It would be easy to assign the reason by discussing the methods employed by each of the three philosophers whom we have named. But it would be requisite to enter into details which might become tiresome. Besides, the experiments to which we allude apply only to temperatures below 100°, and it is beyond that point that we require particularly to know the real dilatation of mercury. It became of course necessary to have recourse to new methods. The one which we shall now describe appears to us susceptible of all the precision that can be desired.

It is founded on this incontestible law of hydrostatics, that when two columns of a liquid communicate by means of a lateral tube, the vertical heights of these two columns are precisely the inverse of their densities. If then we could measure exactly the heights of two columns of mercury contained in the two branches of a reversed glass syphon, while the one was kept at the temperature of freezing water, and the other raised to any determinate known temperature, it would be easy to deduce from this the dilatation required.

If h and h' denote the vertical heights of the two columns producing equal pressures, at the temperatures t and t', we ought to have (calling d and d' the corresponding densities):

$$h\,d = h'\,d'$$

But d and d' are inversely as the volumes v and v', which the same mass of liquid would occupy at the respective temperatures t and t'. Hence we have

$$v' = v\,\frac{h'}{h}$$

From which we deduce for the mean coefficient of the dilatation between $t°$ and $t'°$

$$\frac{h' - h}{h\,(t' - t)}$$

Hence the whole is reduced to the exact measurement of the temperatures and of the heights of the columns; and it is needless to say that we obtain in this way the absolute dilatation of the liquid; since the form of the vessels producing no influence upon the pressure of the liquids contained in them, their dilatation cannot produce any effect.

Boyle first pointed out the use which might be made of this principle for comparing the density of liquids with each other. Several philosophers have thought since his time of applying it to the measurement of dilatations; and it is probable that this very accurate method might be easily applied at low temperatures. But when we wish to apply it at temperatures of 300°, it becomes very laborious.

To render the explanation of the apparatus which we employed more clear, we have drawn a perspective view of it (Plate LXXXIX, fig. 1), in which only the essential pieces are seen, the remainder being capable of being easily supplied.

The recurved tube which contains the mercury consists of the two vertical branches, A B and A′ B′, communicating with each other by a horizontal tube, B B′, carefully made, and preserving in its whole extent the same thickness of glass, and the same inside diameter. Care was taken by a preliminary trial to ascertain that the pressure was transmitted without obstacle from one of the columns to the other by means of the horizontal tube, and that the friction of the mercury against its inside did not prevent the level from being restored when the equilibrium had been disturbed.

Each of the vertical branches is formed, as may be seen in the figure, by an assemblage of two tubes, of very different calibre, cemented together. By giving to the lower tube a small diameter, the total mass of mercury is much diminished; and by terminating it by a larger tube, we avoid the error of the unequal capillary action from the different temperature of the two columns.

The horizontal tube lies (all its length) upon a strong iron bar, M N, in the form of a T, which is itself supported very firmly by its three feet on a very thick wooden table. The upper face of the bar was carefully smoothed, and carries two levels at right angles with each other, which are regulated by means of screws placed at the four corners of the table.

Near each of the vertical tubes rises an iron bar, carrying a ring with a screw, which takes hold of the tube, and keeps it in a fixed position. (Not to overcharge the figure, the bar on the side of the tube, A B, only is drawn. It terminates, as is seen, by an arch of iron, the point of which, R, is intended for a mark.)

The bent tube being thus completed in all its parts, it remained to place the apparatus in such a way as to communicate to each of the two columns the requisite temperature. This was easily done for the column, A B, which was to be kept at zero. It was surrounded with a large cylinder of tin plate, cemented at the bottom round the iron bar, and which was filled with pounded ice to the height of the mercury in the tube. There was a small window, F, in this cylinder, which was opened to disengage a little the pieces of ice, in order to be able to perceive the top of the column of mercury at the time of observation. Accurate thermometers, plunged at different times into this column, showed that it was always exactly at zero.

The part of the apparatus, which was to contain the bath destined to heat the column, A′ B′, was of difficult execution. A box, the bottom of which was of a piece with the sides, could not have answered, because it could not have admitted the column, A′ B′. It was requisite likewise that the bar, M N, should traverse this box, and that the small spaces between the bar and the sides of the box should be filled with an impermeable lute. To satisfy all these conditions, we got a cylinder of copper, whose bottom could be removed at pleasure. It terminates above in an edge, on which the cover is put. It has likewise at its bottom two opposite appendices, R R′, S S′, having each the form of a horizontal semicylinder, in the inside of which passes the bar, M N. An exact idea of it may be formed by inspecting fig. 2, which represents a section of it made by a vertical plane, parallel to the direction of these appendices. The form of the bottom is represented in fig. 3. It was united to the sides of the box by a great number of steel screws forced very tight. But this pressure not being sufficient to prevent the liquid from running out, thin slips of cord were introduced between the two metallic surfaces.

The advantage of the appendices is to allow us to lute at a considerable distance from the fire. But in spite of this precaution, the lute gets hot, and would at last be detached, unless care were taken to cool it perpetually by a current of water.

The box thus constructed was solidly fixed on a furnace, supported on all sides by iron bars. This furnace, in the figure, is supposed to be cut in two, that we may see the pieces in its inside.

We shall terminate this preliminary description by saying, that the copper cylinder is filled with a fixed oil, which is gradually heated till it reach the requisite temperature. Then all the mouths of the furnace are shut; the heat then spreads itself uniformly through the whole mass, and the temperature remains stationary during a time sufficient to take all the requisite measures. But that nothing may alter the exactness of these determinations, it is necessary that the copper be always completely filled with oil, and that the hot column of mercury terminate at a very small height above the cover. We easily fulfil this second determination by adding, or withdrawing by means of a sucker (pipette), the requisite quantity of mercury, some instants before the observation. As to the first, it is obtained by filling the vessel with oil, when cold, and by putting at the top of the vessel, a tube, L Q, whose orifice, Q, is on a level with the under side of the cover. Through this tube the oil flows out as it dilates.

Let us now proceed to the measurement of the temperatures, and of the heights of the columns.

The oil bath contains two thermometers, the one mercurial and analogous to that which we have had occasion to describe already, and in which the temperature is calculated by comparing the weight of the mercury which has made its escape from the instrument, with that which it contains at zero. Such is the sensibility of that which we employed, that an increase of temperature of one degree made about one decigramme of mercury issue out. Its reservoir, D E, is every where of the same diameter, and is plunged into the oil to the same depth as the column, A′ B′. Of course, it gives the exact mean temperature of the column.

The second is an air thermometer, whose cylindrical reservoir, D′ E′, placed like that of the preceding, is terminated by a very fine tube, E′ G′ H′, curved horizontally beyond the furnace. This tube is united at H′, with a vertical tube, a little larger, and well calibred, which is plunged into the mercurial bath, K′. To regulate this thermometer, the bath was in the first place heated nearly to the boiling point of the oil, while the extremity, K′, of the tube remained open. When the whole excess of air had been driven out by the heat, the orifice, K′, was plunged into the mercury, and by the cooling of the oil, the mercury rose gradually in the tube. It is by measuring the height of this column, at the maximum of temperature, and that of the barometer, that the augmentation of the elasticity of the air is ascertained, whence, by a very simple calculation, the temperature of the air thermometer is deduced. It is scarcely necessary to add, that the tube had been carefully dried, and that for each measurement the correction arising from the capillary depression was made.

The indications of this thermometer add nothing to the precision of those furnished by the mercurial thermometer. But we took that opportunity of again comparing the two thermometers. The results deduced from this comparison entered into the determination of the means inserted in Table I.

It remains now to describe the kind of micrometer which we employed to measure the height of the columns. This instrument (fig. 4) is composed of a thick copper rule, A B, along which moves stiffly, but smoothly, a piece of copper, M N P R S, carrying at its two ends, M and S, two rings, in which a micrometer telescope, O O′, turns, furnished at its focus with a horizontal wire. From the telescope is suspended a very sensible level, the graduated tube of which serves to regulate the optical axis. This piece of copper, M N P R S, is susceptible of two movements, one very rapid, by unscrewing the lateral screw, C; the other very gentle, by turning the adjusting screw, D. The whole instrument turns round a vertical axis, which rests upon a thick triangular plane of copper, furnished with a screw at each of its summits.

The construction of this instrument enables us, as is evident, to measure the difference between the height of two columns, which are not situated in the same vertical. It is necessary for this, after having directed the glass to one of the points, to cause the axis to turn, in order to bring it in the azimuth of the other point. It is then raised or depressed the requisite quantity, which is measured on a scale engraven on the opposite face of the rule, A B, by means of a vernier moved by the piece, M N P R S. A micrometer screw would probably have been preferable had it not been for the rapidity which our experiments required. The vernier enabled us to appreciate the 50th of a millimetre, a degree of precision which we thought sufficient.

To give to this instrument all the requisite exactness, it was necessary that the smallest differences between the two heights should be appreciable; and that in the passage from one observation to another, the glass should preserve its horizontality, or at least that we should be able to appreciate the derangement. The first of these conditions was satisfied by giving the telescope a sufficiently high magnifying power; and as for the second, the particular care with which the micrometer was made, the solidity of the support on which it rested, and which was independent of the rest of the apparatus, might have led us to consider it as satisfied. However, we measured beforehand for the distance at which the telescope pointed, to what difference of height would correspond a change of inclination equal to one degree of the level. This datum was sufficient to enable us to correct the observations in which the level was deranged.

The processes employed for regulating such instruments are too well known to require any details here. It is known that by the requisite turnings of the telescope, both upon itself and on its rings, and by observations in the different azimuths in which it may be placed, by turning the axis of the instrument, we have it in our power to render that axis vertical, and the optical axis of the telescope horizontal.

Let us return now to the apparatus of the dilatation. The micrometer was placed upon a marble plane, T, supported by mason work. The axis of the instrument was at an equal distance from the centres of the tubes, A B and A′ B′, and the point, R. Hence we could measure immediately the excess of the height of this point above the summits of the columns of mercury; that is to say, the heights $r - h$ and $r - h'$, calling r the absolute height of R. To be certain that the refraction across the tubes produced no deviation in the vertical direction, we placed artificial horizons in the centre of each, on which we directed our telescope, and we ascertained that the coincidence of the wire was not altered whether we raised or turned the tube.

Nothing further remained than to ascertain r. But this height remained constant in all the experiments, since the bar supporting the arch, R, was always surrounded with ice. To measure it, a vertical graduated rule was employed, the zero of which was placed upon the iron bar, M N. This rule, constructed for another purpose with very great care, gave the height within the tenth of a millimetre. But the heights measured above the bar, M N, are too great; for h, h', and r, ought to be reckoned from the axis of the horizontal tube. Hence from the height given by the rule, we must subtract half the total thickness of the tube.

To enable the reader to judge of the accuracy to which these different operations lead, let us state one of the measures taken at 100°. The height of the arch, R, above the axis of the horizontal tube was 0·58520 metre, the heights $r - h$, $r - h'$, were respectively 0·03855 and 0·02875; therefore $h = 0{\cdot}54395$, and $h' - h = 0{\cdot}00980$. And consequently the mean coefficient of the absolute dilatation of mercury between 0° and 100° $= \frac{1}{5550}$. We see by this, that an error of two or three tenths of a millimetre on the measure of r would produce only an uncertainty of two or three unities in the denominator of the preceding fraction. Thus by a particular effect of the disposition of our apparatus, those measures susceptible of the least precision can only occasion errors that may be entirely overlooked. Supposing that even the iron bar were a little deranged by the effect of the fire (though we always took care to make it horizontal by means of the levels), it would produce but very little effect upon the final result.

In this respect our apparatus is greatly superior to those employed to determine the dilatation of solids. In them the smallest derangement of the fixed point during the long duration of the experiment, does not merely affect the total length of the rule; the dilatation itself is augmented or diminished, which occasions the most serious errors. We see, on the contrary, that when, in our experiments, the heights, h and h', are affected by the cause of which we spoke, the difference $h - h'$ which measures the dilatation is not so. For it is absurd to suppose that the instrument becomes deranged during the very short interval which elapses between the successive observation of the hot and cold column.

We have collected in the following table the mean results of a great number of observations made in the way just described. The first column contains the temperatures such as they are deduced from the dilatation of air; the second contains the mean absolute dilatations of mercury between freezing water and each of the temperatures indicated in the first column; the third column exhibits the temperatures which we should obtain, on the supposition that the dilatation of mercury is uniform, or, in other words, those which should be indicated by a thermometer formed of that fluid inclosed in a vessel, whose expansion followed the same law as its own.

TABLE II.

Temperatures deduced from the dilatation of air.	Mean absolute dilatations of mercury.*	Temperatures indicated by the dilatations of mercury supposed uniform.
0°	0	0·00°
100	$\frac{1}{5550}$	100·00
200	$\frac{1}{5425}$	204·61
300	$\frac{1}{5300}$	314·15

(*To be continued.*)

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* Each of the results contained in this column is the mean of a great number of measures, which it would have been too tedious to have given in detail: we shall satisfy ourselves with giving the extreme values for each of the three temperatures.

	Maximum.	Minimum.
100°	$\frac{1}{5547}$	$\frac{1}{5552}$
200	$\frac{1}{5419}$	$\frac{1}{5431}$
300	$\frac{1}{5289}$	$\frac{1}{5309}$

Article VI.

Defence of Dr. Murray's New Theory of Acids. By John Murray, M.D. F.R.S. Edin.

(To Dr. Thomson.)

SIR, *Edinburgh, Dec.* 8, 1818.

In the account which you give in your number for Dec. of the mutual action of sulphurous acid and sulphuretted hydrogen, and n which an important experimental result is established, you emark of the compound which you find to be formed of these wo gases, that though containing both oxygen and hydrogen, united to a combustible base, it possesses the properties of acidity in a very weak degree, and you consider this as affording a proof that my notion of the greatest degree of acidity being given to bodies by the joint union of oxygen and hydrogen is not countenanced by chemical facts, nor consistent with the phenomena of the science.

Unwilling to engage in controversial discussion, I should not probably have alluded to the subject, with the view merely of obviating an objection. But the fact becomes more interesting when it affords, as I am led to believe it does, an important illustration and confirmation of the truth of my opinion.

In a memoir read at the close of last session before the Royal Society of Edinburgh, on the Relation of the Law of Definite Proportions to the Constitution of Acids, and which I have published lately as an appendix to the new edition of my System of Chemistry, I had given the example of sulphuric acid (oil of vitriol) as affording an argument in support of my views. It is composed of 100 of sulphur with 150 of oxygen, and 56·7 of combined water; that is, of 100 of sulphur with 200 of oxygen, and 6·7 of hydrogen. Sulphurous acid is a compound of 100 of sulphur with 100 of oxygen. The proportion of 200 of oxygen, therefore, in sulphuric acid is the regular multiple conformable to the usual law. The proportion of hydrogen is that which constitutes sulphuretted hydrogen. It appears, therefore, I remark, that the proportions of both these elements are determined by their relation to the sulphur as the radical of the acid, and are those which the quantity of sulphur would separately require. This, so far as theory can discover, is not a necessary result. The oxygen and hydrogen might each have required the quantity of sulphur with which they combine; that is, the existing relations might have been those of sulphur to oxygen, and sulphur to hydrogen, in their several proportions. It is otherwise; there is the relation of sulphur to oxygen, and in addition to this of hydrogen to the same sulphur. And thus, since the same quantity of sulphur receives the acidifying influence of both elements, we discover the source of the higher degree of acid power. How water should augment acidity, no principle enables us to conjecture. But how the joint operation of two elements acting on the same quantity of radical, which each of them separately is capable of rendering acid, should augment the effect, is easily perceived. And even from this consideration alone, there can remain little hesitation in admitting the conclusion, that both these elements act directly on the sulphur; in other words, that the three are in simultaneous combination.

The fact you have discovered is precisely that which I had thus observed might possibly exist, and the conclusion from which was anticipated by the theory. One portion of the sulphur is in that relation to the oxygen which constitutes sulphurous acid, and *another portion of sulphur* is in that relation to the hydrogen which constitutes sulphuretted hydrogen. No augmentation of acidity, therefore, is to be expected; but, on the contrary, from the reciprocal action of the oxygen and hydrogen, rather a diminution below the mean acid power which is displayed in the two binary compounds.

Without, I trust, indulging any undue confidence, I cannot but think that chemists will perceive the fallacy of the opinion that the acids contain combined water, and the much greater probability of the opinion that the elements rather of this water exist in the combination, and from their acidifying influence, produce the important effects, which, without any principle, and in opposition to all analogy, are ascribed to water itself. The constitution of the vegetable acids, in all of which carbon may be regarded as the radical, acidified by different proportions of oxygen and hydrogen, affords even a better illustration of the opinion than the compounds of sulphur; and the view which I have given of them, conformable to this in the same paper, removes, if I am not mistaken, some of the difficulties which attend the subject, and which you have noticed in another paper, on the weights of the atoms of bodies, in the same number of your Journal. With much respect, I remain, Sir,

Your most obedient servant,

J. MURRAY.

ARTICLE VII.

Letter of M. Beudant to M. Arago on the Subject of Dr. Wollaston's Memoir, inserted in the Annals of Philosophy, xi. 283.*

SIR,

I HAVE read with great interest in the number of the *Annals of Philosophy* for April, which you were so good as to send me, a paper by Dr. Wollaston on my memoir, entitled "On the relative Importance of Crystalline Forms and Chemical Composition in the Classification of Mineral Species." I regret very much that I have received it just the evening before my departure for a long journey, and that this circumstance prevents me from entering into some new details which are due to the high consideration in which I hold the celebrated philosopher who has taken the trouble to repeat my experiments. But though obliged to write in haste, I request you to accept of a few observations which the reading of his note has suggested to me.

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* Translated from the Ann. de Chim. et Phys. vii. 399.

I find in Dr. Wollaston's letter three points in which he has given an opinion different from that which I have advanced in my memoir.

1. He affirms that the primitive form of sulphate of iron is not a rhomboedron, but an oblique prism with a rhomboidal base. He founds his opinion on theoretical considerations. "On examining," says he, "the modifications it assumes in its less simple state, I have remarked a manifest difference in one direction of the crystal, proving that if the angular measures were really equal, still the solid could not be considered as a rhomboid; but must be viewed as a rhombic prism on account of some difference in its linear dimensions." He then found by direct measurement that the angles are unequal.

I cannot admit the first part of this statement. It appears to me that if the angles are equal, the solid is rigorously a rhomboid, whatever its extent may be in one direction or another.

As to the second part, the difference of the angles which Dr. Wollaston announces, cannot, as he himself observes, in the least diminish the accuracy of my results, which he has in other respects found exact, and from which he draws the same consequences as I do. I may, therefore, admit the primitive form of sulphate of iron to be as he conceives it. But as the question is respecting one of the essential characters of this salt, I think it better to enter into a short discussion respecting it.

I have not time to verify the inequality of the angles with all the care which this discussion demands. But such measures as I have just taken with the common goniometer give me results sensibly equal; and certainly I could not, with this instrument, commit an error of two degrees, as would result from the observations of Dr. Wollaston. But I shall confine myself here to theoretic considerations, which are much more important than direct measures. In fact it is not by measures, the accuracy of of which depends upon the perfection of our instruments, that we are entitled to pronounce that the crystalline system of a body belongs to a rhomboid or a prism; but by the degree of symmetry which exists in all the faces, whether primary or secondary, which the crystals of the body presents. But in the sulphate of iron, we see all the modifications of which the crystals are susceptible, equally placed two and two, three and three, or six and six, in relation to the same line, which passes through two solid opposite angles. This appears to me *very constant*, both in all the crystals which I have myself obtained, or in those which our manufactories of this salt daily produce. The figures which M. Haüy has given of this salt (plate 79) are perfectly accurate, and it is sufficient to cast the eye on them to see that the faces marked *n, o, s, r, z,* are all arranged relative to an axis which joins the two solid acute angles. This axis, therefore, indicates a pyramidal system of crystallization (if the expression be allowable), and which, therefore, is quite foreign to a system of an oblique prism. When this last system of crystallization exists, the symmetry of the modifications is quite different. The faces which modify the prevailing form are never arranged relatively to an axis passing through two opposite solid angles, and of course the edges, or angles, on which these modifications take place, have not that symmetry which a rhomboid presents.

The expression *linear dimensions*, which Dr. Wollaston employs, seems to indicate that he has taken into consideration the relative dimensions which the edges of the crystals obtained, present. This I recognize in a subsequent part of the letter, when he mentions an experiment quite similar to one of those which I gave in the last memoir that I presented to the Academy of Sciences (Recherches sur les Causes qui font varier les Formes Crystallines d'une meme Substance Minerale), in which he obtained, as I did, very elongated sulphate of iron mixed with copper. He says on this subject, "*the prismatic form is so elongated that it shows evidently that it is not a rhomboid.*"

I cannot by any means agree to this opinion. A rhomboid may be elongated in one direction; it then exhibits a kind of oblique prism with a rhomboidal base. But though the edges are unequal in their dimensions, the rhomboidal character does not exist the less; but is recognized in the symmetry of the modifications. In fact, I obtain at pleasure these same crystals, elongated, and modified by additional faces; and I recognize obviously that all these faces are placed symmetrically with respect to one axis—a character which accurately characterizes the rhomboidal system.

2. Dr. Wollastom seems to think that I consider the crystals which I obtained as *mechanical* mixtures of different other salts, in a manner analogous to the gres de Fontainebleau. I never entertained any such idea; and one of the notes attached to my memoir shows this sufficiently. I think likewise that we may regard these associations as combinations; but since they take place in variable proportions, it was necessary to distinguish them from combinations in definite proportions. This was the reason why I adopted the expression *chemical mixture*.

3. Dr. Wollaston mentions experiments in which he dissolved

together sulphate of copper and sulphate of zinc, both quite free from iron; and he says that he obtained crystals which had the form of sulphate of iron.

The analogous experiments which I had made, and of which I have already given an idea in my memoir, proved to me that the crystals, similar to those of sulphate of iron, contain all of them traces of this last salt; and I am tempted to believe that those obtained by Dr. Wollaston contained it likewise. But to discover it, we must analyze a considerable quantity of these crystals. When I employed sulphate of zinc and sulphate of copper, prepared with the greatest care, and which, when examined in considerable quantities, gave no trace of sulphate of iron, I never obtained any thing else than crystals of sulphate of copper or sulphate of zinc.

After having thus stated the opinions which I think should be adopted, and after having given precision to those that I formerly advanced, I will add that I perfectly agree with Dr. Wollaston relative to the form of sulphate of nickel. It is certainly a symmetrical octahedron with a rectangular base; or, if that is preferred, a right prism with a rectangular base. The crystals which M. Haüy examined were called sulphate of nickel by Leblanc, who, in this point as well as in others, has not examined the results with sufficient care. His crystals undoubtedly belong to the double sulphate of potash and nickel: their primitive form is an oblique rhomboidal prism. This is demonstrated by the particular symmetry which the different crystalline forms of this salt present, of which I have obtained several very beautiful.

The observation which terminates Dr. Wollaston's letter, in which he describes crystals of sulphate of nickel in small octahedrons, cemented by the double sulphate of nickel and potash, appears to me of the greatest importance for the theory of the mixtures of different salts with the preservation of the form of one of them. It agrees perfectly with all the ideas suggested to me by the numerous experiments which I have made on the subject.

I finish by testifying to Dr. Wollaston how much I am flattered that my experiments drew his attention. I am anxious that the new experiments, which I have just presented to the Academy, may likewise merit it; and that he will have the goodness to enlighten by his observations the new route which I have endeavoured to traverse. I am, &c.

F. F. Beudant.

Article VIII.

Meteorological Observations made at Cork. By T. Holt, Esq.

(With a Plate. See XC.)

(To Dr. Thomson.)

Sir,

Cork, Oct. 12, 1818.

I transmit you the meteorological report for the third quarter of 1818; and remain, with due respect, Sir,

Your obedient humble servant,
Thomas Holt.

REMARKS.

JULY.

1. Cloudy day; light showers.
2. Dull, dry day.
3. Cloudy morning; bright day; cloudy evening.
4, 5. Bright days; fresh breeze.
6. Fair, but cloudy.
7. Rainy morning; fine day.
8. Fine, bright day.
9. Showery morning and evening; fair day.
10, 11, 12. Fair, but cloudy.
13, 14. Bright, hot days.
15. Foggy morning; bright day.
16. Bright day; cloudy evening.
17. Cloudy, close day.
18. Bright morning; thunder and lightning, with heavy rain at noon; showery evening.
19. Cool, cloudy day; breeze.
20. Light showers through the day.
21. Fine day.
22. Misty and showery day.
23. Bright day.
24. Light rain last night; heavy showers this day.
25. Rainy morning; fair from noon.
26. Mild day; light showers.
27. Bright day.
28. Showery morning; rainy day.
29. Rain last night; cloudy day.
30. Cloudy day, with showers; rainy evening.
31. Showery morning; fine day.

AUGUST.

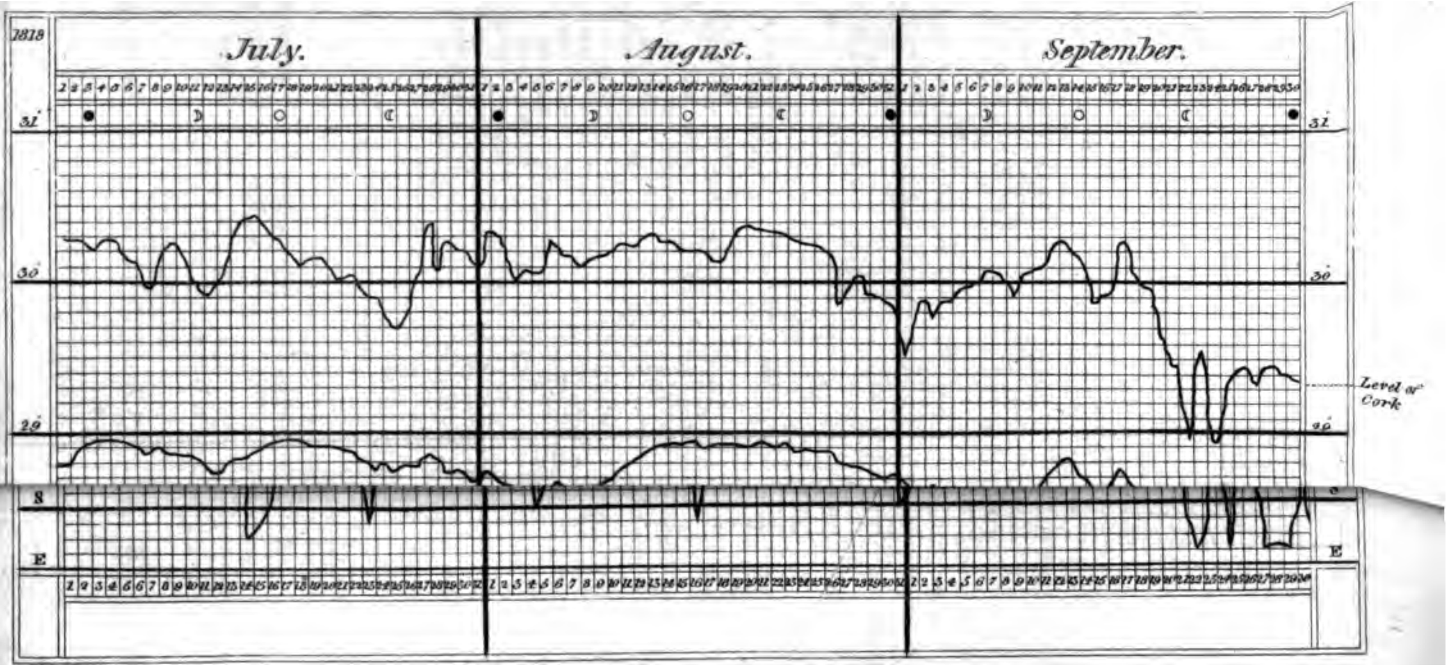
1. Cloudy, dry day.
2. Ditto gale.
3. Rain last night, and misty day.
4. Dry, cloudy day.
5. Ditto, rainy evening.
6, 7. Ditto.
8. Ditto rain last night.
9. Rain last night; bright day.
10. Clear, bright day.
11. Cloudy, fine day.
12, 13, 14, 15. Clear, fine days.
16. Cloudy, dry day.
17. Bright day.
18. Ditto, showers.
19, 20, 21, 22, 23. Cloudy; breeze.
24. Light showers; cloudy.
25, 26. Ditto, ditto; breeze.
27. Rain last night; showery morning; dry day.
28. Cloudy; light showers at night.
29. Cloudy day; rainy evening.
30. Bright day.
31. Dry day; rainy evening.

SEPTEMBER.

1. Rain last night; fine day; rainy evening; gale.
2. Bright day; gale.
3. Rainy morning and showery day.
4, 5, 6. Showery.
7, 8. Showery days; frosty nights.
9, 10, 11, 12. Bright day; breeze.
13. Misty till noon; then cloudy.
14. Rain last night; fine day; rainy evening.
15. Ditto; cloudy day; rainy evening.
16. Fine day; a few light showers.
17. Bright day.
18. Fine day; occasional showers; gale at night.
19. Rainy morning; dry, cloudy day
20. Fine day.
21. Rain, with high wind.
22. Fine; occasional showers; thunder and lightning, with rain at night.
23. Violent rain, with heavy gale through last night; and several heavy showers this day.
24. Showery day; calm.
25. Fair; breeze.
26. Showery; calm.
27, 28. Rain night and day.
29. Dry, cloudy day; breeze.
30. Rainy day.

Plate XC.

Barometer.



RAIN.

18.	Inches.	1818.	Inches.	1818.	Inches.
uly 7	0·331	Aug. 3	0·072	Sept. 1	0·288
9	0·036	5	0·060	3	0·241
18	0·024	9	0·030	4	0·060
20	0·213	18	0·045	5	0·009
22	0·041	24	0·003	6	0·017
24	0·048	27	0·051	7	0·066
25	0·309	29	0·015	8	0·030
28	0·138	31	0·219	14	0·042
29	0·141			15	0·144
31	0·216		0·495	16	0·048
				18	0·063
	1·497			19	0·243
				21	0·325
				22	0·198
				23	0·729
				24	0·270
				26	0·126
				27	0·441
				28	0·390
				30	0·378
					4·108

ARTICLE IX.

On the Formation of the Rainbow. By Robert Watt, M.D.

(To Professor Thomson.)

DEAR SIR, *Glasgow, Nov.* 4, 1818.

About fifteen years ago I was engaged in a variety of searches respecting the nature of light and heat, which led me pay particular attention to what have been called the primitive lours, and more especially as they appear in the rainbow. For time I was satisfied with the Newtonian theory, and of course my views were bounded by that hypothesis. The first thing ich tended to stagger my belief in it was that I could, in few stances, satisfy myself that there were really drops of rain ling at the place where the rainbow appeared. A rainbow s often seen in a direction in which for many miles not a drop rain had fallen. It occurred to me, therefore, that to make Newtonian theory complete, the existence of drops of rain uld first of all have been demonstrated.

Without troubling you with my doubts and difficulties, I shall rtly state what gave me an entire new view of the subject. e day, I think in 1805, while I was observing a very vivid nbow, I happened to turn my eyes towards the sun, and erved that he was passing along the lower edge of a sort of semitransparent cloud. I could distinctly see his boundaries on the lower side; but the cloud becoming more and more dense, the upper part of his disk was scarcely visible. Matters remaining in this situation for some minutes, I had time to make repeated observations on the sun, the cloud through the edge of which he was shining, the dark sky in the east, and the variations in the rainbow. The cloud passed slowly to the north, and the sun appeared in all his splendour. At this instant the rainbow disappeared, though I could perceive no difference whatever in the dark sky on which it had so lately been seen.

The coincidence of the sun's emerging from under the cloud, and the disappearance of the rainbow, struck me as remarkable, and led me to conclude that probably there was some connexion between them. Can this cloud have acted as a prism in refracting the rays? Perhaps the dark sky on which the rainbow was seen, served no other purpose but a curtain to receive the spectrum. Full of this idea, I waited with great impatience till I had an opportunity of seeing another rainbow. When this occurred, it convinced me still more of the correctness of my hypothesis. The moment I saw the rainbow, I turned round, and saw a cloud between me and the sun, and the sun shining through its lower edge as before. The lower edge of the cloud being somewhat uneven, at times more of the sun's disc was covered than at others, and corresponding variations always took place in the appearance of the rainbow. At one pretty large gap the sun shone forth unclouded, and the rainbow disappeared. In a minute or two he was partially covered, and the rainbow again made its appearance. At last the cloud passed wholly off, and the rainbow was seen no more. Though from this time I considered my hypothesis as in a great measure established, I missed no opportunity of looking for such a cloud as often as I have seen a rainbow, and I have never in one instance seen the one without finding the other.

I cannot say what are all the conditions necessary to produce a rainbow; but the following are so constant, that I will venture to predict a rainbow is never seen without them—the sun shining through the edge of a cloud, and a dark sky in the opposite direction to receive the spectrum. Without the least visible change in the sun or in the curtain, all the changes in the rainbow may be foreseen and foretold by marking the motions of the intervening cloud. My attention was recalled to this subject by a friend telling me a few days ago that he had lately seen a most striking proof of my theory of the rainbow. A rainbow appeared and disappeared repeatedly as the sun was more or less covered by the edge of a cloud. I had myself an opportunity, within these two weeks, of witnessing the same thing at sea. By observing for a short time the motion of the cloud, I predicted to those around me that the rainbow, which had continued for *several* minutes, and was still as bright as ever, would not be seen above two minutes longer, which was the case. The sun passed rapidly from under the cloud, and the rainbow as instantly disappeared.

As the above hypothesis is new, at least so far as I know, and as numerous opportunities must occur to almost every person of judging how far I am right or wrong, I shall thank you to give it a place in your publication. I am, dear Sir,

Your most obedient servant,

ROBERT WATT, M.D.

ARTICLE X.

ANALYSES OF BOOKS,

Memoirs of the Wernerian Natural History Society. Vol. II. Part II. For the Years 1814, 1815, 1816.

(*Continued from p.* 66.)

VIII. *An Account of several new and rare Species of Fishes, taken on the South Coast of Devonshire, with some Remarks upon some others of more common Occurrence.* By George Montagu, Esq. F.L.S. and M.W.S.

Col. Montagu found that the female of the raia clavata, or thornback, has blunt teeth, and possesses the other characters usually given to this species by systematic writers. But the male raia clavata has sharp pointed teeth smaller than those of the female. The hooked spines are peculiar to the male. Hence our author conceives that the male of the raia clavata has been described as a distinct species under the name of *raia fullonica.* The *raia rubus*, or *rough ray*, he thinks, is another variety of the male thornback.

The author gives a particular description of the *raia chagrinea*, or shagreen ray, first described by Mr. Pennant. He points out the mistakes that occur in the last edition of the British Zoology, and in Shaw's Zoology, in the description of this species, and gives the characters by which it may be readily distinguished from other species; and he gives an accurate figure of the fish which accompanies the paper.

Valuable observations are given on the raia *oxyrinchus;* the raia *maculata,* which has been described by some under the name of raia *rubus;* by others under the name of raia *miraletus*; and the raia *microocellata.* This last ray is distinguished from the other species by the smallness of the eyes. Both it and the R. *chagrinea* are called Duncow by the fishermen in the west of England.

The *ziphotheca tetradens* was described by Col. Montagu in the first volume of the Wernerian Memoirs as a new species of fish. He thinks that the fish described by Dr. Shaw in his General Zoology, and placed by him in the thoracic order under the name of Vandellius lusitanicus, is the same fish. It was the inaccurate position of this fish by Shaw that prevented him from recognizing the identity sooner. Risso, in his *Ichthyologie de Nice*, has described three species belonging to this genus, under the generic title Lepidopus, but has placed them inaccurately in the thoracic order.

Col. Montagu gives a figure and an accurate description of the Leplocephalus Morrisii, first described by Pennant, though afterwards its existence was called in question. The author obtained two specimens of this rare fish, from his friend Mr. Anstice, of Bridgewater, near which place they had been taken. From these specimens, one of which was quite perfect, he has been enabled to draw up a much more accurate description than had been previously given by Pennant, whose specimen had been incomplete.

In the first volume of the Wernerian Memoirs, Mr. Neill relates his observations on the *Callionymus lyra* and *dracunculus*, from which he concluded that the difference between these two fishes is merely sexual, the former being the male, and the latter the female. Col. Montagu is induced to throw some doubts upon the accuracy of this conclusion, from the circumstance of the callionymus dracunculus being very common upon the coast of Devonshire. The fishermen of Torcross alone catch above 1000 of them annually; but the callionymus lyra is very rarely met with in that quarter. Col. Montagu only procured one specimen, which a fisherman sent him as a rare fish, with which no body in his neighbourhood was acquainted. He requests the secretary of the Wernerian Society to go on with his dissections at different seasons of the year till the sex of the callionymus dracunculus be ascertained; for at the time that he published his paper in the first volume of the Wernerian Memoirs, on the fishes in the Frith of Forth, he had not been able to find traces either of roe or melt in that fish.

The *blennius ocellaris* is well known as a Mediterranean fish, but was not supposed to occur on the coast of Great Britain; but three of them were taken by the dredge in 1814 on the oyster bed at Torcross on the south coast of Devon. The author had an opportunity of examining them all, and one of them indeed in a living state. He gives a figure and description of this fish, and points out the mistakes respecting it into which preceding ichthyologists had fallen.

The author next gives a correct description of the *blennius gattorugine*, and gives some valuable characters by which certain species of blennius may be distinguished from the rest.

The remainder of this valuable paper is taken up with remarks on the *gadus argenteolus*, a small species of gadus which has been hitherto confounded with the *gadus mustela*: with an account of the *sparus lineatus*, of which a figure and accurate description is given: with a description of a new species of gurnard, which he distinguishes by the name of *trigla lævis*. The paper terminates with remarks on the trigla cuculus and the trigla lineata.

IX. *Observations upon the Alveus, or general Bed of the German Ocean and British Channel*. By Robert Stevenson, Esq. Civil Engineer.—This paper contains a very particular and curious detail of the wasting effects of the sea upon the coasts of Scotland, which the author, from his official situation, as inspector of the northern light-houses, has had the annual means of ascertaining for these several years past. He states, and indeed the fact is notorious, that the sea has within these few years past washed away a good deal of land from the south shore of the Frith of Forth, and from various other parts of the coast both of Scotland and England which he enumerates. This wearing away of the coast he ascribes to the gradual filling up of the channel of the German Ocean. The consequence of the continual deposition of matter washed from the dry land by the action of the rivers must, he conceives, have a tendency to fill up the bottom of that ocean, and of course to raise its level. Mr. Stevenson is disposed to generalize this, and to consider it as general all over the globe; so that, in his opinion, the level of the ocean over the whole of our globe is every where rising.

This rise of the level of the ocean, in consequence of the deposition of the detritus of the dry land into its bed, was the foundation of Dr. Hutton's theory of the earth. The accuracy of the conclusion was disputed with much zeal by Deluc and Kirwan, and defended with great eloquence by Prof. Playfair. Without entering into so intricate a controversy, it may be sufficient to observe that Mr. Stevenson's arguments prove too much. If the devastations upon the coasts of Great Britain are owing to the filling up of the channel of the German Ocean and the consequent rise of the surface of that ocean, this filling up of the bed of that ocean must be going on with prodigious rapidity. I myself remember perfectly since the road between Newhaven and Leith went much within the present high water mark, and since there was a space of ground between it and the sea. I remember, and many of the inhabitants of Edinburgh and Leith must likewise remember, the violent storm by which this piece of land was swept away; and from the nature of the soil in that part of the coast, when the wasting process has once begun, it is likely to go on for a considerable space. But that there is not the least alteration in the height of the surface of the Frith of Forth is quite obvious from the marks upon Leith pier; for the tide rises no higher on that pier at present than it did 20 years ago. Probably indeed no perceptible change has taken place in that height for centuries; for some of the harbours on the north coast of the Frith seem to have remained unaltered for several hundred years. I have no doubt that the sea is shallower in different parts of the coast of Great Britain at present than formerly. But this change can be very well accounted for by local causes, which, in most cases, are sufficiently obvious. The author has not taken into his consideration many examples that might be adduced even upon our own coast of the land gaining on the sea. If the surface of the ocean had really risen, no one instance of that kind could possibly exist.

X. *Geological Remarks on the Cartlone Craig.* By Dr. Macnight.—This is a vast chasm in the sandstone near Lanark, and constitutes a most beautiful piece of scenery. Dr. Macnight, who seems to have entered most thoroughly into the spirit of the Wernerian geognosy, is of opinion that this chasm has been formed by the subsidence of a portion of the sandstone rock.

XI. *Account of the Irish Testacea.* By Thomas Brown, Esq. F.L.S. M.W.S. M.K.S.—This catalogue contains 239 species, of which about 12 appear to be new. It seems to be drawn up with much ability by a gentleman very well acquainted with the branch of natural history to which he has devoted his attention. Capt. Brown considers Ireland as richer in shells than either England or Scotland. The catalogues of Irish sea shells, he says, is imperfect, in consequence of his residing while in Ireland at a distance from the coast.

XII. *Remarks respecting the Causes of Organization.* By Dr. Barclay.—The author was led to the observations in this paper from perusing a description of a monstrous fœtus, by the late Dr. Sandifort, Professor of Physic, Anatomy, and Surgery, in the University of Leyden. This fœtus wanted all the bones of the cranium, except those which constitute the base. Instead of a brain it had a soft substance, differing from that organ in form, magnitude, and colour. Dr. Sandifort was of opinion that the brain had once existed in this and similar monsters, and that it had afterwards disappeared in consequence of some accidental injury. But Dr. Barclay conceives that we have no evidence for this: and that in many cases, as where the head, the head and neck, the head, neck, and shoulders, or the intestines, are wanting altogether, no such supposition can be formed. Living beings, he says, originate from certain liquids secreted in the organs of the parents. Something in these liquids (that is, the *living principle*) begins the formation of appropriate organs, and by these organs, when once formed, the connexion between the living principle and the external world is maintained.

The subject of a living principle is the most difficult department in science. Much has been written upon the subject, and many opinions, sufficiently whimsical and ridiculous, have been advanced respecting it, but little or no real progress has been made in the discussion. The opinions of modern physiologists differ from those of their predecessors; but they do not seem to be supported by better evidence. It is not difficult, therefore, to predict the fate which several opinions, at present sufficiently fashionable, and considered as plausible or established, are destined to meet with from posterity. Dr. Barclay has devoted much of his time to the study of the living principle, and has at present a work upon it ready for the press. I have no doubt that when it appears, it will do him credit; and that it will contain a full and impartial review of all the opinions that have been advanced regarding it.

XIII. *On the Genera and Species of Eproboscideous Insects.* By William Elford Leach, Esq.

XIV. *On the Arrangement of Œstrideous Insects.* By William Elford Leach, Esq.

These papers being entirely technical, do not admit of abridgment.

XV. *Observations on some Species of the Genus Falco of Linnæus.* By James Wilson, Esq.—This is a learned and amusing paper, and well entitled to the attention of ornithologists. The genus Falco is one of the most obscure departments of natural history; several different names being frequently given to the same species, the male and the female being often described as distinct species, and the old bird in like manner distinguished from the young. Mr. Wilson is of opinion that the Falco chrysaetos, the golden eagle, is a distinct species from the Falco fulvus, or ring-tailed eagle, though several modern French ornithologists have confounded them together. His reasons seem to be very good for considering them as distinct species. The terms Falco apivorus, Falco albidus, and Falco variegatus, belong, he informs us, to the same species, the Honey buzzard.

The term *gentle,* or *gentil,* is applied by falconers to falcons that have been properly tamed and trained. The term *haggard* is applied to those falcons that have been taken by the lure, and not having been sufficiently tamed are apt to fly away after rooks and pigeons.

Mr. Wilson points out several mistakes into which authors have fallen with respect to the Falco gentilis, which is in fact nothing else than the common falcon. He gives a description of the Falco palumbarius, or goshawk, and of the Falco communis, or common falcon, of which he describes no fewer than 12 varieties.

XVI. *On the Geognosy of the Lothians.* By Prof. Jameson. —This paper, I conceive, has inadvertently got a wrong title. The author has intended it as an introduction to a geological account of the Lothians; and probably when he began to write the paper he intended not to stop short at the introduction, but to give likewise the geological account to which the title refers. But as nothing more has been printed at present but the introduction, which has little reference to the Lothians, it would have been better if the title of the paper had been "On the Red Sandstone of the Middle District of Scotland."

That portion of Scotland to which Prof. Jameson gives the name of the middle district is bounded on the north by the Caledonian canal, and on the south by the Frith of Forth. A great variety of formations exist in it both primitive, transition, and floetz. But the red sandstone covers perhaps a greater portion of it than any other formation; and it has been very carefully and skilfully examined by our author, who is, without exception, the most industrious and indefatigable geologist in Scotland; devoting the three autumnal months of every year to the examination of some tract of country or other.

The red sandstone in the middle district of Scotland stretches from Stonehaven, in Kincardineshire, to the west side of the island of Arran, and in some places extends in breadth to many miles. The Ochils and the Seadley Hills are both situated in the red sandstone, and constitute each a pretty long range of hills. These hills are composed of rocks very different in their nature from red sandstone; but as they lie in that formation, Prof. Jameson considers them as constituting so many subordinate formations to the red sandstone. Nor is this the only alteration in the Wernerian geognosy which has been the consequence of the examination of the structure of Great Britain.

Werner distinguished the sandstone formation by the name of *old red sandstone,* and considered it as the oldest of the floetz formations, and as lying immediately over the primitive, or transition rocks. Red sandstone in Great Britain may be traced from Kincardineshire, in Scotland, as far south as Devonshire. One portion of this sandstone, the portion which exists in Scotland and in the north of England, is obviously below the coal beds, and immediately in contact with primitive or transition formations. It is, therefore, the *old red sandstone of Werner.* But in the south of England, as in Warwickshire, Worcestershire, &c. it lies as obviously above the coal beds. On this account two distinct red sandstone formations have been distinguished by British geologists under the names of *old red sandstone* and *new red sandstone.* Mr. Jameson seems inclined to suspect that these two sandstones constitute in fact but one great formation, and that the coal beds are in reality only beds occurring in red sandstone, and subordinate to it. If this supposition were to be adopted, it would be necessary to reduce all the floetz formations of Werner below the chalk to *old red sandstone,* and to consider almost all the floetz formations of Werner as subordinate to the red sandstone. This seems to me to approach to the opinion entertained by Mr. Whitehurst, and founded upon a survey of the midland district of England. Such a sweeping generalization would be undoubtedly very convenient for Wernerian geologists, as it would enable them to overleap every difficulty that could be started against their peculiar opinions; but whether it would be attended with advantages sufficient to induce us to adopt it, is a question which deserves some consideration. The subordinate formations of Werner were a very ingenious thought, and enabled him to generalize the structure of the earth much more easily, and to make it much more interesting than would have otherwise been possible. He was never embarrassed by the appearance of a subordinate formation. He had merely to assign the great formations in which it was apt to occur. Then it might be indifferently present, or absent, as far as the theory was concerned. The point was to establish the great general formations, which included the subordinate ones. But if we affirm that there is only one or two floetz formations, and that all the other formations that occur in floetz districts are subordinate to these two, is not this merely another mode of giving up the regularity of the structure of the crust of the earth, and tacitly affirming that there is no regularity whatever in the order in which the different rocks follow each other? If greenstone, porphyry slate, compact felspar, &c. occur in old red sandstone, and in the newest floetz trap formations, and in all the intermediate formations, and if there be no criterion by which these rocks can be distinguished from each other in these different positions, it seems clear that the occurrence of these rocks can give us no information of the part of the series in which they occur. We may find greenstone below coal or above coal, or in places not in the least connected with coal; so that the occurrence of greenstone can give us no information whether the tract of country in which we find it be situated below the coal or above the coal. The same remark may be applied to all the other rocks which constitute only subordinate formations. I conceive, therefore, that it will be worth Prof. Jameson's while to consider whether the prodigious extent which he is inclined to give to subordinate formations be not a virtual acknowledgment that the order of the different rocks constituting the crust of the earth, is less regular than Werner thought it to be. I do not wish him to abandon the doctrine of general formations. If it can be shown that there exist no more than five general formations; namely,

1. Granite.
2. Gneiss and mica slate.
3. Clay slate.
4. Sandstone.
5. Chalk.

And if all the other rocks be subordinate to these, this will be at least a very material point ascertained. I think it likely that geologists would come sooner to correct results if they were to begin by assuming those formations only to be general which are observed covering very large tracts of country, and in the most opposite parts of the earth.

Professor Jameson's account of the red sandstone of Scotland is highly instructive, and merits the closest attention of geologists; but like all technical descriptions, it is of a nature not susceptible of abridgement. The following are the names of the subordinate rocks which this red sandstone contains.

Conglomerate,
Slate clay,
Claystone,
Clay ironstone,
Trap tuff,
Amygdaloid,
Basalt?
Clinkstone,
Compact felspar,
Porphyry,
Greenstone,
Pitchstone,
Limestone,
Limestone conglomerate,
Coal.

Article XI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

The annual meeting for the election of officers for the ensuing year took place on Nov. 30, when the following noblemen and gentlemen were elected:

President.—Right Hon. Sir Joseph Banks, Bart. G.C.B. &c.
Secretaries.—W. T. Brande, Esq. and Taylor Combe, Esq.
Treasurer.—Samuel Lysons, Esq.

There remained of the old Council, Right. Hon. Sir. J. Banks, Bart.; W. T. Brande, Esq.; Lord Bishop of Carlisle; Taylor Combe, Esq.; Sir H. Davy, Bart.; Sir E. Home, Bart.; S. Lysons, Esq.; George, Earl of Morton; John Pond, Esq.; W. H. Wollaston, M.D.; T. Young, M.D.

There were elected into the Council, J. P. Auriol, Esq.; R. Bingley, Esq.; Sir T. G. Cullam, Bart.; John, Earl of Darnley; S. Davis, Esq.; Sylvester, Lord Glenbervie; Major-Gen. Sir J. W. Gordon, K.C.B.; Sir A. Johnston, Knight; Rev. R. Nares; Sir G. T. Staunton, Bart.

At this meeting, the Copley medal was voted to Mr. R. Seppings, for his various improvements in the construction of ships, communicated to the Royal Society, and published in their Transactions.

Jan. 14.—A paper, by Sir E. Home, was read, on the Corpora Lutea. The texture of the ovarium before puberty is loose and open, and contains globular cells. After puberty, the corpora lutea are found in the substance of the ovarium. In the cow, they form a mass of convolutions, which Sir E. compared to those of the brain. The ova are formed in the corpora lutea; and, according to our author, exist previously to, and independently of, sexual intercourse; and when the ova are formed, the corpora lutea are destroyed by absorption, whether the contained ova are impregnated or not. Sir E. thinks that impregnation is necessary to the expulsion of the ova, and that the corpus luteum is burst by extravasated blood, its cavity after the escape of the ovum being found distended with blood in a coagulated state. When impregnation does not take place, the ovum remains in the cavity of the corpus luteum. Hence the author thinks it probable that the ovum is impregnated in the ovarium itself.

Beautiful drawings, illustrative of these points, accompanied the paper, founded chiefly on the observations of Mr. Bauer, who assisted Sir Everard in the present inquiry.

GEOLOGICAL SOCIETY.

Nov. 6.—A paper, from William Phillips, Esq. M. G. S. "On the Chalk Cliffs, &c. on the Coast of France, opposite Dover," was read.

The appearance from Dover of the cliffs on the opposite coast of France, induced Mr. Phillips to suspect that they might resemble in their formation those of the English coast which he had lately described; and on crossing the channel, examining the strata from Sandgate to St. Pot, he found them to consist of deposits similar to those which constitute the long range of coast between Dover and Folkstone, except that the upper part of the bed with numerous flints is not visible on the French coast. The dip of the strata appears the same on both sides of the channel, but the thickness as well as the height of the cliffs is much less on the French side. Hence, although the strata became thinner in that portion which now constitutes the French coast, Mr. Phillips considers that they were once continuous with the English beds, and formed a part of what is now termed the chalk basin of London, the then connecting mass having been since washed away by the action of the sea.

A paper from N. S. Winch, Esq. containing sections of the coal formations in Northumberland, was read.

A paper from William Phillips, Esq. on the modifications of the primitive crystal of sulphate of barytes, was read.

The primitive crystal, a right rhomboidal prism, the angles of which were found by the reflecting goniometer to measure 78° 18′ and 101° 42′, is subject to modifications on its acute and obtuse edges, and on all its solid angles. Mr. P. has observed 18 modifications, and he has described the secondary planes produced by them, of which an illustrative series of figures is given with the paper.

Dec. 4.—A paper was read, from Dr. Davy, communicated by Sir James M'Gregor, on the Geology and Mineralogy of the Island of Ceylon.

This island consists almost entirely of one mass of primitive rock, composed chiefly of gneiss and dolomite, rising in some places to 7000 feet above the level of the sea—an elevation which Adams' Peak, the highest mountain in the island, does not exceed. With the exception of one spot, the shores are generally shelving. The mountains and plains are generally covered with accumulated debris, and the soil, which is poor, corresponds to the rocks from which it is derived; the water is pure, and of the mean annual temperature of the place where it rises, except in the neighbourhood of Trincomalee, where there are hot springs of 103° to 137°.

The valleys are in general narrow and deep, with outlets which render them incapable of forming lakes; but there are some salt lakes formed by the sand banks thrown upon the sea-shore.

Besides the two species of rock before spoken of there are masses of granite, both common and graphic, sienite, felspar, rock, and greenstone.

Iron in different forms is general in the island, but no other metal has been discovered: its poverty, however, in this respect is not less remarkable than its richness in rare and valuable gems, which are scattered through the alluvial ground, but are seldom found in their native rock. The minerals which have been observed by Dr. Davy are quartz, cat's-eye, prase, hyalite, &c. Tourmaline, garnet, pyrope, cinnamon-stone, zircon, hyacinth, spinelle, sapphire, and common corundum, several varieties of felspar, mica, carbonate and anhydrous sulphate of lime, apatite, graphite, and ceylonite. No traces are visible of volcanic action in any part of the island.

A communication was read from Ed. L. Irton, Esq. of Irton Hall, on a third sand-tube found at Drigg.

The remains of this tube were discovered about 10 or 15 yards nearer to the sea than the former ones. Passing through about four feet of pebbles, its course was continued nearly eight feet through wet sand; it then became much contorted and irregularly formed, sometimes being solid, and again becoming tubular, and terminating at a granite pebble with only a small diverging ramification extending but a few inches.

SOCIETY FOR THE ENCOURAGEMENT OF ARTS, MANUFACTURES, AND COMMERCE.

We hope in future to present our readers with notices of the more important communications made to this Society, and to which the Society has voted rewards. Since the commencement of the present session, in November last, rewards have been adjudged for the following interesting subjects.

Mr. W. Hardy's Inverted Pendulum.—This valuable instrument has already been made known to the public by Capt. Henry Kater, F.R.S. in a late paper in the Transactions of the Royal Society, containing "An Account of Experiments for determining the Length of the Pendulum, vibrating Seconds, in the Latitude of London;" who derived considerable advantage from it, in proving the stability of the support for his endulum. It has also received the approbation of other eminent nen in the scientific world. The Society adjudged its gold sis medal, to be presented to Mr. Hardy, for this invention.

Mr. Einslie's Ivory Paper.—This paper possesses a surface, having many of the valuable properties of ivory, and at the same time has the superior advantage of being obtained of a much greater size than ivory can possibly furnish, even nearly as large as the usual sheets of drawing paper. The Society has voted the sum of 30 guineas to Mr. Einslie for this invention.

Mr. Alexander Bell's New Chuck for a Lathe.—This instrument can be screwed into, or upon, the mandrel of a lathe, and has three studs projecting from its flat surface, forming an equi-lateral triangle, and which are capable of being moved equably to, or from, its centre. These studs are provided with teeth, and can be made to embrace, or enclose, any hollow, or solid, circular body between them, within the extent of its limits, and retain it firmly, in order to turn, bore, or operate, in any other manner upon it in the lathe. From the greater simplicity of its construction, it can be made much cheaper than similar contrivances for the same purpose. The Society awarded its silver medal and the sum of 10 guineas to Mr. Bell for this invention.

ARTICLE XII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Method of determining the Specific Gravity of the Gases.*

The apparatus necessary for taking the specific gravities of gases, by the following method, consists of a delicate balance, or rather beam, so constructed that two bulky vessels of exactly the same size and weight may be conveniently suspended from its extremities. One of these vessels may be a globe, or flask, furnished with a stop-cock as usual, and of any convenient size. The other must be cylindrical, so as to admit of being graduated; say, into 1000 equal parts; and must be likewise furnished with a stop-cock having an extremely minute aperture.

The two vessels, as before stated, must be exactly of the same size and weight, so that, when filled with atmospheric air and suspended, the index of the beam shall stand at 0, and these easy adjustments are the whole that are required. When used, the globe, or flask, is to be filled with the gas, whose specific gravity is to be determined in the usual manner, and the cylindrical vessel is then to be so far exhausted as to be rendered *lighter* than the globe, or flask, thus filled. Both vessels being now suspended, one at each extremity of the beam, the stop-cock of the cylindrical vessel is to be opened, and so much air be permitted to enter by the minute aperture above-mentioned as shall be requisite to bring the two vessels in the same exact equilibrium as at the commencement of the experiment. The cylindrical vessel is then to be removed from the beam, and its stop-cock opened under mercury, and thus the precise *bulk* of air contained in it be accurately measured, which bulk (if the whole vessel has been graduated to 1000 parts) represents the specific gravity of the gas weighed, common air being 1000. Thus, suppose hydrogen to be the subject of experiment, and it be found that 69·44 parts of common air be equal in weight to 1000 parts of hydrogen, the specific gravity of hydrogen will be ·06944 common air by 1·000, or it is 14·4 times lighter than common air.

The above form of the apparatus is more particularly adapted for determining the specific gravity of gases *lighter* than common air; but it is obvious that the principle upon which the method is founded is equally applicable to gases *heavier* than air, by a little modification in the apparatus. The advantages of the method are many and important. Besides the greater general accuracy attainable by *measuring* than *weighing* gases, the use of weights, as well as the necessity of ascertaining the bulk and weight of the apparatus, as in the common mode of determinig the specific gravity of gases, are entirely superseded; nor are errors likely to arise from any change in the atmospheric temperature, or pressure, occurring during the performance of the experiment.

II. *Sulphate of Strontian.*

This substance has been lately found in considerable quantity at Carlisle, about 34 miles west of Albany, state of New York, imbedded in clay slate, forming very extensive strata. It was first tried by a common smith as a substitute for borax, and has been found the most useful flux ever employed in brazing and welding. By employing a very small quantity of it in powder, instead of clay, he welded easily the most refractory steel; and in brazing, it proved superior to borax, on account of its remaining more fixed at a high temperature.

III. *Subterranean Noises.*

At Haddam, in Connecticut, for several years past, noises, like the firing of small arms, have been continually heard, which have been accompanied with almost continual concussions of the earth. So frequently have these effects been experienced, that they are quite disregarded by the inhabitants. About six years since, however, a serious explosion took place, which rent and dislocated large masses of the granite mountains.

IV. *Scientific Expedition in America.*

A scientific party will proceed in March to explore the natural productions of the numerous large rivers tributary to the Mississippi. They will go in a steam-boat now building for the purpose at Pittsburg; and expect to be absent for upwards of three years. T. Say, Esq. of Philadelphia, will be one of the party.

V. *Mineralogy in America.*

We observe in the American papers proposals from Dr. J. W. Webster for a course of lectures on mineralogy and geology at Boston; and are happy to observe the science acquired in the university of our northern metropolis, during the intervals of professional studies, becoming actively employed in a district, we apprehend, abounding in minerals in considerable variety.

VI. *Africa.*

Mr. T. E. Bowditch, who has recently published his travels in Africa, is about to return to Cape Coast Castle, accompanied by Messrs. Williams and Salmon, surgeons. These gentlemen are all good naturalists; and will make frequent excursions into the interior with the view of exploring its natural history.

VII. *Temperature of Bombay.*

An account of the state of the barometer and thermometer, &c. in this island for 1816 and 1817, was given in the *Annals* for Sept. last by Mr. Knight. The following additional observations recently published, though made long before, may not be altogether devoid of interest.

Mean temperature of the island as estimated from the averages of observations made during a period of two years, viz. 1803 and 1804.

	Morn.		Noon.		Night.
1803.—	$79\frac{3}{4}°\ \frac{22}{30}$	……	$82\frac{3}{4}°\ \frac{13}{30}$	……	$81°\ \frac{21}{30}$
1804.—	$79\ \frac{18}{30}$	……	$82\ \frac{11}{30}$	……	$80\frac{1}{4}\ \frac{22}{30}$
Average	$79\frac{1}{2}\ \frac{5}{30}$		$82\frac{1}{4}\ \frac{27}{30}$		$80\frac{3}{4}\ \frac{10}{30}$

General average of 1803. ……… $81\frac{1}{4}°\ \frac{8}{30}$

General average of 1804. ……… $80\frac{1}{2}\ \frac{9}{30}$

Mean temperature …… $80\frac{3}{4}\ \frac{28}{30}$

The morning observations were generally made between six and eight o'clock, the noon between 12 and 4, and the evening between half-past 9 and 12, and the *greatest* height at noon was noted when several observations were made. The thermometer is stated to have been placed out of the direct influence of the sun about 23 feet above the level of high water mark.

It appears that the greatest diurnal range of the thermometer during the above period varied from $5\frac{1}{2}°$ to $13\frac{1}{2}°$, the least diurnal range occurring from April to October, and the greatest from November to March. With respect to the above mean temperature, the author observes that it is a more favourable one than from observations in other parts of India or of the world in the same latitude, we should have been warranted in supposing, and that the morning average in particular can hardly be relied upon as conveying an accurate idea of the morning temperature.

The following table presents the number of rainy days in 1803 and 1804, years remarkable for the difference in their great leading features, the first being a year of unusual scarcity, the second of uncommon abundance.

	1803.—Days of		1804.—Days of	
	Heavy rain.	Showers.	Heavy rain.	Showers.
Jan.	—	—	—	3
Feb.	—	2	—	—
March.	—	—	—	—
April	—	—	—	—
May.	—	—	—	2
June	14	11	11	11
July.	14	14	17	13
August	15	8	7	17
September.	2	3	14	14
October	—	5	—	6
November	1	1	—	—
December	—	—	—	—
	46	44	49	66
		46		49
General total		90		115

The author considers the difference of the fall of rain in the months of September to have been the chief cause of the above-mentioned difference between the crops of the two years.—(Abstracted from a paper by Lieutenant Colonel Jasper Nicholls in the Transactions of the Literary Society of Bombay.)

VIII. *Population of Bombay.*

The whole population of Bombay, at the period below-mentioned, was estimated to vary from 160,000 to 180,000. Of this number, about $\frac{1}{8}$ were Mussulmen, $\frac{1}{16}$ of Parsee caste, and $\frac{1}{31}$ Christians; the remainder were chiefly Hindoos, who thus constituted the great bulk of the inhabitants. The following is a general account of the number of deaths from 1801 to 1808 inclusive. It is founded on returns made to the police office of the number of bodies buried or burned in the island.

1801	4,835	1805	10,347
1802	5,297	1806	6,440
1803	8,320	1807	5,834
1804	25,834	1808	7,517

"The average deaths during the year would, by this account, be 9,000, or about 1 to 19; but the year 1804 in which the deaths are nearly trebled, was a season of famine throughout the neighbouring provinces on the continent of India. Great multitudes sought refuge from death at Bombay; but many of them arrived in too exhausted a state to be saved by the utmost exertions of humanity and skill. This calamity began to affect the mortality in 1803; and its effects are visible in the deaths of 1805."

From other data, it appears that the average of the deaths of the Mahometan sects during 1806, 1807, and 1808, were to their whole numbers as 1 to 17½, of the Parsees as 1 to 24, and of the Christians in different districts between 1 to 22, and 1 to 16.

With respect to the relative proportion of males to females in Bombay, it appears that the number of males exceed in general that of females throughout all the different sects comprising the population of the island (except the Christians, and for which no cause is assigned)—an insuperable argument against the necessity of polygamy, especially when taken in conjunction with similar well-authenticated facts. Indeed this practice appears to be very limited, and to be confined almost exclusively to the rich; for it is stated that out of 20,000 Mahometans in Bombay, only about 100 have two wives; and only five have three; so inconsiderable, continues the author, "is the immediate practical result of a system, which in its principles and indirect consequences produces more evil than perhaps any other institution." —(Abstracted from note to discourse delivered at the opening of the Literary Society of Bombay by Sir J. Mackintosh.)

IX. *Gezangabeen, or Persian Manna.*

This substance, to which various origins have been assigned, is found chiefly in Persia and Arabia. Capt. E. Frederick, of the Bombay establishment, states, that the *gez* of which he supposes the *gezangabeen* is formed, is found on a shrub, resembling the broom, called the *gavan,* which he describes as growing "from a small root to the height of about two feet and a half, and spreading into a circular form at the top, from three to four feet and a half in circumference. The leaves were small and narrow; and underneath the *gez* was observed, spread all over the tender branches like white uneven threads, with innumerable little insects creeping slowly about.

"These insects were either of three species, or the same in three different stages of existence. The one was perfectly red, and so small as to be scarcely perceptible; the second dark, and very like a common louse, though not so large; and the third a very small fly. They were all extremely dull and sluggish, and fond of lying or creeping about between the bark of the gavan and the *gez.*" This substance is stated to be collected every third day for 28 days about the month of September.

Capt. F. made the above observations near the town of Khonsar, where, and in Looristan, this substance is chiefly found. He states that the *gez* is obtained by beating the bushes with a stick. When first separated, it is a white sticky substance, not unlike hoar frost, of a very rich sweet taste. It is purified by boiling, and then mixed up with rose water, flour, and pistachio nuts, into cakes, and in this form constitutes the sweetmeat called in Persia *gezangabeen*, and which, by the Persians, is highly valued. Though the *gez*, when first collected, admits of being sifted, still in its original state it is brittle and adhesive at the same time—qualities for which it is remarkable after its preparation as a sweetmeat. If pressed, it sticks to the fingers; but on being smartly struck, separates easily into small grains, like sugar. It is in this state in cool weather; but above the temperature of 68°, it liquefies, and resembles white honey both in colour and taste.

Besides the above species of manna, other products of a similar nature are stated by the author of the present paper, as well as others, to be found in Persia and the neighbouring countries.—(Transactions of the Literary Society of Bombay.)

Meerza Jiăfer Tabeeb, a Persian physician, now in London, gives a different account of this substance. *Gez*, according to him, is the name of a tree called in Arabic *Turfā*, and which is supposed to belong to the Tamarisk genus. Of this tree there are two species; one a shrub, which yields the substance in question, called *gezangabeen* (a term meaning literally *juice of the* (tree) *gez*), used only as a sweetmeat; the other, a tree yielding a somewhat similar substance, called in Arabic *Athel*, and which is employed in medicine as an astringent. Besides these two species of manna, he states they have a third, called in Arabic *Terenjubin*, which is used as a laxative. This gentleman also states, that it is the universal opinion in Persia that all these varieties are exudations from the trees on which they are found, and not the work of insects.

X. *Information respecting the Tree called Lignum Rhodium in Pococke's Travels.* By Sir James Edward Smith, M.D. Pres. L. S.

Pococke in his well-known Description of the East, ii. 230, speaking of Cyprus, has the following passage:

"Most of the trees in the island are evergreen; but it is most famous for the tree called by the natives Xylon Effendi, the Wood of our Lord, and by naturalists *Lignum Cyprinum* and *Lignum Rhodium*, because it grows in these two islands. It is called also the Rose Wood, by reason of its smell. Some say it is in other parts of the Levant, and also in the isle of Martinico. It grows like the *platanus*, or plane tree, and bears a seed and mast like that, only the leaf and fruit are rather smaller. The botanists call it the oriental plane tree. The leaves being rubbed have a fine balsamic smell, with an orange flavour. It produces an excellent white turpentine; especially when any incisions are made in the bark. I suppose it is from this that they extract a very fine perfumed oil, which, they say, as well as the wood, has the virtue of fortifying the heart and brain. The common people here cut off the bark and wood together, toast it in the fire, and

, or oriental liquidambar.
Sibthorp in his visit to Cyprus was anxious to ascertain
e mentioned by Pococke. He found it still growing, and
erated by the natives, though not quite so much so as it had
the time of Pococke. It was the *liquidambar styraciflua,*
North American species. No other tree of this species
own in the island of Cyprus, nor probably in the Levant.
ins, therefore, as a problem difficult of solution to account
first planting of this tree in the island of Cyprus.—(Lin-
ransactions, xii. 1.)

ower of the Sarracenia Adunca to entrap Insects. By Dr. James M'Bride.

e 12th volume of the Linnæan Transactions, p. 48, there
ous communication from Dr. Macbride, of South Carolina,
property which the leaves of the *Sarracenia flava* and
have of entrapping insects. These plants grow abun-
in the flat country of South Carolina. The leaves are
, and several feet in length. In the months of May,
r July, when these leaves perform their extraordinary
ns in the greatest perfection, if some of them be removed
use and fixed in an erect position, it will soon be per-
that flies are attracted by them. These insects imme-
approach the fauces of the leaves, and leaning over their
appear to sip with eagerness something from their
surfaces. In this position they linger; but at length
as it would seem, by the pleasure of taste, they enter the
The fly, which has thus changed its situation, will be
stand unsteadily; it totters for a few seconds, slips, and
the bottom of the tube, where it is either drowned, or
s in vain to ascend against the points of the hairs. The
om takes wing in its fall and escapes. But this sometimes
s, especially where the hood has been removed to assist
tion. In a house much infested with flies, this entrap-
es on so rapidly that a tube is filled in a few hours, and
nes necessary to add water, the natural quantity being
ient to drown the imprisoned insects.

ause which attracts flies is evidently a sweet viscid sub-
resembling honey, secreted by, or exuding from, the
surface of the tube. On splitting a leaf, it may readily
overed in front, just below the margin, and in greatest
y at the termination of the *ala ventralis*. From the margin
t commences, it does not extend lower than one-fourth
nch. During the vernal and summer months, it is very

perceptible to the eye and the touch; and although it may be sometimes not discoverable by either, yet the sensation of sweetness is readily perceived on applying the tongue to this portion of surface. In warm and dry weather, it becomes inspissated, resembling a whitish membrane. The falling of the insect as soon as it enters the tube is wholly attributable to the downward or inverted position of the hairs of the internal surface of the leaf. At the bottom of a tube split open, the hairs are plainly discernible pointing downwards; and as the eye ranges upwards, they become gradually shorter and attenuated, till at, or just below, the surface covered with the bait, they are no longer perceptible to the naked eye, nor to the most delicate touch. It is here that the fly cannot take a hold sufficiently strong, but falls.

The putrid masses of insects collected in the leaves of these plants, probably serve some purpose beneficial to the growth of the vegetable; but what that purpose is would not be an easy task to conjecture.

XII. *British Species of Roses.*

It appears from a paper by Mr. Joseph Woods, in the 12th volume of the Linnæan Transactions, p. 159, that the indigenous British species of roses amount to 26. The following are the names of these species as given by Mr. Woods.

1. Rosa cinnamomea. Found near Pontefract.
2. —— rubella. Northumberland and Scotland.
3. —— spinosissima.
4. —— involuta. Arran and west of Scotland.
5. —— doniana. Found by Mr. G. Don on the mountains of Clova.
6. —— gracilis. Villosa of English Botany. Darlington.
7. —— Sabini. Near Dunkeld.
8. —— villosa. Mollis of English Botany. Near Edinburgh.
9. —— scabriuscula. Northumberland and Scotland.
10. —— heterophylla. Near Edinburgh.
11. —— pulchella. Ingleton, Yorkshire.
12. —— tomentosa.
13. —— nuda. Near Ambleside, Westmoreland.
14. —— Eglanteria. Rubiginosa of English Botany. Kent.
15. —— micrantha.
16. —— Borreri. Near Edinburgh.
17. —— cæsia. Argyleshire.
18. —— sarmentacea.
19. —— bractensis. Ulverton, Lancashire
20. —— dumetorum.
21. —— collina.
22. —— hibernica. In Ireland.
23. —— canina.
24. —— surculosa. Sussex and Kent.
25. —— systilla. Collina of English Botany.
26. —— arvensis.

XIII. *Effect of Common Salt on the Solubility of Nitre in Water.*

A curious set of experiments on this subject has been recently published by M. Longchamp. I shall here state some of the most remarkable facts which he has ascertained.

At the temperature of 39°, the specific gravity of a saturated solution of nitre and common salt is 1·3057. It is composed of

Water	61·74
Nitre	16·06
Common salt	22·20
	100·00

Now 61·74 parts of water, of the temperature 39°, are capable of dissolving only 9·823 parts of nitre; so that the solubility of the nitre was increased by the presence of the common salt in the ratio of 153 to 100. Probably at lower temperatures, the solubility of nitre in water would be doubled by the presence of common salt.

At the temperature of 64½°, the sp. gr. of a saturated solution of nitre in distilled water is 1·151. It is composed of

Water	78·37
Nitre	21·63
	100·00

The following table exhibits the effect of the addition of common salt (added in different proportions) upon the power of such a solution to dissolve additional quantities of nitre. The temperature is always supposed to be 64½°. The first column gives the quantity of solution of nitre employed; the second that of common salt added; the third that of the nitre dissolved, in consequence of the presence of the common salt; the fourth that of the nitre in solution in the liquid employed; the fifth the total of saltpetre in the liquid, including both the original quantity and the new quantity rendered soluble by the common salt. The sixth column gives the specific gravity of this compound liquid, containing both nitre and common salt.

Quantity of solutions of nitre employed.	Common salt added.	Nitre dissolved by means of the common salt.	Nitre in the original solution.	Total nitre dissolved.	Sp. gr. of the solutions.
Grammes.	Grammes.	Grammes.	Grammes.	Grammes.	Grammes.
100	5·00	0·746	21.63	22·376	1·1871
100	10·00	1·267	21·63	22·897	1·2212
100	15·00	1·658	21·63	23·288	1·2523
100	20·00	1·827	21·63	23·457	1·2832
100	25·00	2·583	21·63	24·213	1·3096
100	26·85	3·220	21·63	24·850	1·3290

M. Longchamp considers this increased solubility of the nitre as occasioned by the mutual decomposition of the two salts by each other.—(Ann. de Chim. et Phys. ix. 10.)

XIV. *Constituents of Saltpetre.*

In a late number of the *Annals of Philosophy,* I inserted a set of experiments on the analysis of this salt by Berthollet; and contrasted his results with those of Dr. Wollaston, and my own. These three sets of experiments were made in a different way. Dr. Wollaston saturated a given weight of bicarbonate of potash with nitric acid, and determined the weight of the nitre formed. Berthollet decomposed a given weight of saltpetre by heat, and measured the volume of oxygen and azotic gases evolved. I decomposed a given weight of nitre by sulphuric acid, and determined the weight of the sulphate of potash formed. It is probable that none of these methods is susceptible of absolute precision. But if each of them were performed with as much accuracy as the experiment would admit, the mean of the results obtained by the three methods would, in all likelihood, give us the true result. Dr. Wollaston's method has been recently repeated by M. Longchamp. He found nitre composed of

Nitric acid	53·297
Potash	46·703
	100·000

(Ann. de Chim. et Phys. ix. 27.)

This result does not differ much from that obtained by Dr. Wollaston; namely,

Acid	53·243
Potash	46·457
	100·000

If an atom of nitric acid weigh 6·75, and an atom of potash 6, as I conceive them to do, then the true composition of nitre must be,

Acid	53·726
Potash	46·274
	100·000

Dr. Wollaston's numbers approaching more nearly to these than the numbers given by Longchamp, I consider them as nearer the truth. Indeed Dr. Wollaston's results do not differ $\frac{1}{500}$th part from the theoretical composition of nitre. Now I am afraid that it is scarcely possible to come nearer the truth than this by a single direct experiment. Chemical precision, like astronomical, can be looked for only from the mean of a great number of experiments so contrived that the errors must of necessity fall on different sides.

XV. *Morphia.*

An account of the original experiments of Sertürner on the infusion of opium, his method of extracting morphia and meconic acid from that infusion, and the subsequent trials of Robiquet and Vogel, have been already given in this journal. But no account has yet appeared in English of the results obtained by M. Franz Anton Choulant, though they have been published at least a year ago in Gilbert's Annalen (lvi. 342). As these experiments are the most minute, and probably the most precise that have been yet made, I shall in this and some of the following notices state the principal facts contained in his paper.

1. *Method of procuring Morphia.*—Four ounces of well-dried and pounded opium were digested in repeated quantities of cold distilled water till the liquid amounted to the quantity of about 16 English pints. This infusion was evaporated by a gentle heat on the sand-bath in a glass vessel till it was reduced to eight ounces. The whole was then poured into a porcelain evaporating dish. After standing at rest for eight hours in a temperature between 54° and 77°, six grains of small crystals were deposited, which possessed the properties of sulphate of lime. The whole, being evaporated to dryness, was redissolved in four ounces of distilled water, with the exception of a small quantity of brownish-coloured resin. Oxalate of ammonia being dropped into the solution, it became muddy, and a precipitate fell, which weighed, when dry, 3½ gr. After this precipitate had been separated, muriate of barytes was added, as long as it occasioned a precipitate. This last precipitate, being separated and dried, weighed two grains.

The solution was now diluted with eight pints of distilled water, and caustic ammonia poured in as long as any precipitate continued to fall. The precipitate thus obtained was white and flocky. After standing two hours, it became granular and brown. It weighed six drams. It dissolved completely in eight ounces of distilled vinegar, and was precipitated by caustic ammonia without any alteration in its colour or its weight. Upon this precipitate, one ounce of sulphuric ether was poured; the mixture swelled up considerably. It was thrown upon a white paper filter. In the course of an hour and a half, a deep black liquid ran through, which weighed half an ounce. It had a strong ammoniacal smell, burned very readily, and left a bulky charcoal behind it.

The matter remaining upon the filter had a frothy appearance; but, when dry, it was in the state of a very fine powder, and had lost much of its dark colour. It now weighed 4⅔ drams. This powder was digested three times in caustic ammonia, and as often in alcohol. Both of these liquids acquired a dark-brown colour, and left the *morphia* in the state of a brownish-white powder, reduced to the weight of three drams.

This powder was dissolved in 12 ounces of boiling alcohol. The filtered solution being set aside for 18 hours, deposited colourless, transparent crystals, consisting of double pyramids. These crystals weighed 75 gr. and consisted of morphia in a state of purity. The alcoholic solution being evaporated to two ounces, deposited one dram of morphia, similar to its state before its solution in the alcohol. When still further concentrated, 15 gr. of yellow-coloured morphia were obtained. The crystals of morphia thus obtained contained no traces of ammonia.

2. *Properties of Morphia.*—It crystallizes in double four-sided pyramids, whose bases are squares, or rectangles. Sometimes in prisms with trapedoizal bases.

It dissolves in 82 times its weight of boiling water, and the solution on cooling deposits regular, colourless, transparent crystals.

It is soluble in 36 times its weight of boiling alcohol, and in 42 times its weight of cold alcohol of 92°.

It is soluble in eight times its weight of sulphuric ether.

All these solutions change the infusion of Brazil wood to violet, and the tincture of rhubarb to brown.

They have a bitter and peculiar astringent taste, and the saturated solutions of morphia in alcohol and ether, when rubbed upon the skin, leave a red mark.

The following are the salts of morphia examined by Choulant.

(1.) *Sulphate of Morphia.*—It crystallizes in prisms, dissolves in twice its weight of distilled water, and is composed of

Acid	22	5·00
Morphia	40	9·09
Water	38	
	100	

(2.) *Nitrate of Morphia.*—Needle-form crystals deposited in stars. Soluble in 1½ times its weight of distilled water. Constituents,

Acid	20	6·75
Morphia	36	12·15
Water	44	
	100	

(3.) *Muriate of Morphia.*—Feather-shaped crystals and needles. Soluble in 10½ times its weight of distilled water. Constituents,

Acid	35	4·625
Morphia	41	5·132
Water	24	
	100	

(4.)—*Acetate of Morphia.*—Crystallizes in needles. Soluble in its own weight of water. Constituents,

Acid	36	6·375
Morphia	44	7·791
Water	20	
	100	

(5). *Tartrate of Morphia.*—Crystallizes in prisms. Soluble in thrice its weight of water. Constituents,

Acid	42	8·375
Morphia	36	7·178
Water	22	
	100	

(6.) *Carbonate of Morphia.*—Crystals short prisms. Soluble in four times its weight of water. Constituents,

Acid	28	2·75
Morphia	22	2·16
Water	50	
	100	

XVI. *On the Equivalent Number for Morphia.*

From the numbers which I have annexed to the preceding analyses of Choulant, indicating the weight of the atoms of the acids, and the corresponding number for morphia, it is obvious that the analyses are very far from correct; for we obtain a peculiar number for morphia from each salt. These numbers are as follows:

From the sulphate	9·009
nitrate	12·150
muriate	5·132
acetate	7·791
tartrate	7·178
carbonate	2·160

The number from the carbonate differs so far from the rest that we must exclude it. It is obvious that the substance examined must either have been a mixture, or a subsalt. The mean deduced from the remaining five salts, gives us 8·268 for the weight of an atom of morphia. In the present state of the investigation, we may take 8·25 as an approximation to the weight of an atom of morphia; but in all probability it is not a very near one. Choulant's experiments must have been made upon too small a scale to expect accurate numerical results.

Article XIII.

Astronomical, Magnetical, and Meteorological Observations.

By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 42″ North. Longitude West in time 1′ 20·7″.

Astronomical Observations.

Dec. 3.	**Emersion of Jupiter's first satellite**	**5h 19′ 20″**	**Mean Time at Bushey.**
		5 20 41	**Mean Time at Greenwich.**

Magnetical Observations, 1818. — *Variation West.*

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Dec. 1	8h 25′	24° 37′ 27″	1h 20′	24° 48′ 32″	Owing to the shortness of the days, evening observation discontinued.	
2	8 25	24 38 46	1 20	24 41 02		
3	— —	— — —	— —	— — —		
4	8 25	24 35 00	1 20	24 41 10		
5	8 30	24 38 55	1 15	24 40 08		
6	8 35	24 36 00	1 20	24 41 17		
7	8 30	24 36 24	1 35	24 39 23		
8	8 30	24 37 08	1 15	24 39 54		
9	8 30	24 37 47	1 10	24 41 47		
10	8 35	24 35 58	1 15	24 41 11		
11	8 35	24 35 45	1 15	24 38 22		
12	8 35	24 35 17	1 10	24 41 05		
13	8 35	24 37 26	1 15	24 41 35		
14	8 35	24 34 17	1 15	24 41 18		
15	8 40	24 35 48	1 20	24 40 24		
16	8 40	24 36 51	1 15	24 44 09		
17	8 40	24 37 48	1 15	24 43 37		
18	8 35	24 39 55	— —	— — —		
19	8 30	24 38 51	1 25	24 41 08		
20	8 35	24 36 39	1 20	24 40 50		
21	8 35	24 34 41	1 25	24 38 51		
22	8 35	24 38 37	1 20	24 44 04		
23	8 30	24 37 49	— —	— — —		
24	8 35	24 38 32	1 30	24 43 49		
25	8 40	24 47 37	1 20	24 41 12		
26	8 40	24 38 17	1 15	24 40 17		
27	8 45	24 37 19	1 30	24 39 42		
28	8 40	24 40 20	1 15	24 39 56		
29	8 35	24 36 49	1 20	24 40 48		
30	8 40	24 34 50	1 20	24 40 53		
31	8 45	24 35 50	1 15	24 40 52		
Mean for Month.	8 35	24 37 04	1 19	24 41 20		

In taking the monthly mean of the observations, that on the morning of the 25th is rejected, being so much in excess, for which there was no apparent cause.

Meteorological Observations.

th.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Dec.		Inches.				Feet.		
1	Morn....	29·585	46°	68°	S by E		Cloudy	45½
	Noon....	29·500	44	59	SSE		Cloudy	47
	Even	—	—	—	—		—	
2	Morn....	29·531	42	80	NNW		Fine	41
	Noon....	29·540	44	60	N		Cloudy	—
	Even	—	—	—	—		—	
3	Morn....	—	—	—	—		—	38
	Noon....	—	—	—	—		—	—
	Even	—	—	—	—		—	
4	Morn....	29·038	44	81	ESE		Fine	41
	Noon....	28·988	49	60	SE		Showery	49
	Even	—	—	—	—		—	
5	Morn....	29·010	42	70	S		Cloudy	41
	Noon....	29·036	48	64	SSW		Cloudy	49
	Even	—	—	—	—		—	
6	Morn....	29·130	38	84	SE		Fine	37
	Noon....	29·122	47	64	SSW		Cloudy	47½
	Even	—	—	—	—		—	
7	Morn....	28·976	44	94	SSW		Showery	42
	Noon....	29·000	49	91	SSW		Rain	49½
	Even	—	—	—	—		—	
8	Morn....	29·270	47	97	S		Fog, rain	44
	Noon....	29·344	49	64	WSW		Cloudy	50½
	Even	—	—	—	—		—	
9	Morn....	29·571	42	67	NE by E		Cloudy	40½
	Noon....	29·570	44	68	NE by E		Rain	44⅓
	Even	—	—	—	—		—	
10	Morn....	29·685	38	98	NE		Foggy	36
	Noon....	29·680	43	66	NE		Cloudy	43
	Even	—	—	—	—		—	
11	Morn....	29·654	35	68	NNE		Fine	33½
	Noon....	29·656	40	57	NE		Fine	41⅓
	Even	—	—	—	—		—	
12	Morn....	29·669	35	86	N by E		Cloudy	32½
	Noon....	29·640	39	74	N by E		Cloudy	41
	Even	—	—	—	—		—	
13	Morn....	29·679	37	86	ENE		Showery	34½
	Noon....	29·694	40	65	NE		Very fine	41
	Even	—	—	—	—		—	
14	Morn....	29·743	37	95	NNE		Showery	36
	Noon....	29·748	41	66	N		Cloudy	41⅓
	Even	—	—	—	—		—	
15	Morn....	29·700	36	93	NNE		Cloudy	35
	Noon....	29·626	39	54	E by N		Very fine	40
	Even	—	—	—	—		—	
16	Morn....	29·532	27½	76	NNE		Very fine	27
	Noon....	29·582	32	67	NNW		Clear	33
	Even ...	—	—	—	—		—	
17	Morn....	29·648	23	77	W by S		Clear	22
	Noon....	29·595	31	65	SW by W		Very fine	31½
	Even	—	—	—	—		—	
18	Morn....	29·350	28	93	SW		Fine	25
	Noon....	29·270	—	94	W by N		Rain	36½
	Even....	—	—	—	—		—	

Meteorological Observations continued.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Dec.		Inches.				Feet.		
19	Morn....	29·753	30°	91°	NW		Clear	29°
	Noon....	29·788	35	62	WSW		Very fine	40½
	Even	—	—	—	—		—	
20	Morn....	29·644	40	68	SW		Cloudy	31½
	Noon....	29·590	45	69	SW		Sm. rain	
	Even	—	—	—	—		—	47½
21	Morn....	29·625	46	68	WNW		Very fine	44
	Noon....	29·737	45	50	NW by N		Fine	
	Even	—	—	—	—		—	46
22	Morn....	30·038	34	75	SSW		Very fine	33
	Noon....	30·044	40	54	Calm		Very fine	
	Even	—	—	—	—		—	40½
23	Morn....	29·961	32	71	E		Very fine	32
	Noon....	—	—	—	—		—	
	Even	—	—	—	—		—	37½
24	Morn....	29·794	26	72	NE		Fine	23
	Noon....	29·707	29	46	SW		Fine	
	Even	—	—	—	—		—	31
25	Morn....	29·668	30	76	SE by S		Cloudy	24
	Noon....	29·620	33	78	S by W		Foggy	
	Even	—	—	—	—		—	35
26	Morn....	29·400	31	69	SSE		Cloudy	30½
	Noon....	29·393	32	67	S		Cloudy	
	Even	—	—	—	—		—	34
27	Morn....	29·590	34	68	ESE		Fine	31½
	Noon....	29·660	38	63	E by N		Very fine	
	Even	—	—	—	—		—	38
28	Morn....	29·988	35	58	NE by E		Cloudy	32
	Noon....	30·034	39	50	NE		Cloudy	
	Even	—	—	—	—		—	38½
29	Morn....	30·126	31	80	NNE		Cloudy	30
	Noon....	30·115	36	68	NNE		Very fine	
	Even	—	—	—	—		—	36
30	Morn....	30·020	28	80	N		Clear	26½
	Noon....	29·976	31	77	NW by N		Foggy	
	Even	—	—	—	—		—	31½
31	Morn....	29·940	31	97	NW by N		Foggy	26
	Noon....	29·929	33	86	NW		Fine	
	Even	—	—	—	—		—	34

Rain, by the pluviameter, between noon the 1st of Dec. 1818, and noon the 1st of Jan 1819, 1·215 inch. The quantity that fell on the roof of my Observatory, during the same period, 1·231 inch. Evaporation between noon the 1st of Dec. 1818, and noon the 1st Jan. 1819, 0·53 inch.

ARTICLE XIV.

METEOROLOGICAL TABLE.

18.	Wind.	BAROMETER. Max.	BAROMETER. Min.	BAROMETER. Med.	THERMOMETER. Max.	THERMOMETER. Min.	THERMOMETER. Med.	Hygr. at 9 a. m.	Rain.	
Mon.										
c. 21	W	30·50	30·15	30·325	48	24	36·0	76	—	
22	Var.	30·50	30·40	30·450	28	23	25·5	93		
23	N W	30·40	30·27	30·335	35	23	29·0	96		
24	Var.	30·27	30·10	30·185	28	22	25·0	87		
25	N E	30·10	29·82	29·960	38	26	32·0	88		
26	S E	30·05	29·80	29·925	35	30	32·5	72		
27	S E	30·47	30·05	30·260	39	32	35·5	78		●
28	N E	30·60	30·47	30·535	41	30	35·5	69		
29	N W	30·58	30·47	30·525	38	22	30·0	81		
30	W	30·45	30·38	30·415	34	23	28·5	84		
31	N W	30·48	30·38	30·430	38	26	32·0	94		
19.										
Mon.										
n. 1	N W	30·50	30·45	30·475	35	26	30·5	90		
2	W	30·45	30·30	30·375	38	30	34·0	90		
3	S E	30·30	30·15	30·225	42	24	33·0	75		☾
4	S E	30·17	30·11	30·140	40	22	31·0	95		
5	N	30·18	30·15	30·165	40	26	33·0	97		
6	Var.	30·15	29·76	29·955	45	32	38·5	95		
7	S	29·86	29·60	29·730	46	33	39·5	80	—	
8	W	29·86	29·40	29·630	47	33	40·0	81	25	
9	S W	29·87	29·43	30·650	51	34	42·5	72	36	
10	S W	29·80	29·48	29·640	49	40	44·5	75	15	
11	S W	29·60	29·48	29·540	46	34	40·0	98	11	○
12	S W	30·13	29·60	29·865	50	33	41·5	82		
13	S W	30·12	29·95	30·035	49	35	42·0	75	—	
14	S W	30·00	29·85	29·925	53	37	45·0	88	—	
15	S W	30·20	29·77	29·985	50	33	41·5	79	16	
16	N W	30·30	30·00	30·150	50	34	42·0	61	—	
17	S W	30·00	29·25	29·625	50	35	42·5	70	19	
18	N W	29·80	29·28	29·540	43	32	37·5	71		
		30·60	29·25	30·068	53	22	35·86	82	1·22	

observations in each line of the table apply to a period of twenty-four , beginning at 9 A. M. on the day indicated in the first column. A dash s, that the result is included in the next following observation.

REMARKS.

Twelfth Month.—21. Much wind about three, a. m. with a little rain: a very fine day ensued: *Cirrocumulus*, with bright sunshine. 22. White frost: foggy, a. m.: clearer, p. m. with *Cirrus*: rime on the trees. 23. White frost: rime to the tree tops: misty, a. m.: sun very bright at noon: much fog to the south. 24. Very foggy: rime still on the trees. 30. White frost. 31. A very fine day.

1819. *First Month.*—1. Very foggy, with the addition of obscurity from smoke 3. air: rather overcast sky. 4. Much rime on the trees: rather misty air. 5. Somewhat misty: the melted rime forming puddles under the trees. 6. Fine day: at night, small portions of cloud were observed to pass swiftly under the moon. 7. The sun-rise was attended with a veil of *Cirrus* clouds passing to *Cirrostratus*, very red and lowering: about noon came *Cumuli* and other clouds, with a gale and showers. 8. Fine day: night windy, with some rain. 9. Hazy, a. m. with *Cirrostratus* and wind: heavy showers, p. m.: very clear night. 10. Overcast soon after sun-rise, with wind: the fore part of the night very stormy. 11. A wet squall this morning: fair day, with *Cirrus* and wind. 12. Fair morning, with slight hoar frost: the gale has subsided. 13. Slight hoar frost: very fine day: at evening, windy again. 14, 15. Windy, with some showers. 16. A fine drying wind, a. m. with *Cirrus* and *Cirrostratus* in delicate striæ: also transient *Cirrocumulus* at a great elevation: a stormy night followed. 17. A very tempestuous day: the rain ceasing for a while, a. m. I observed *Cirrostratus* around large *Cumuli*, rising and separating, as if the shower had been produced by their previous inosculation: much wind in the night. 18. A fine, drying day, with the wind more moderate, and an overcast sky.

RESULTS.

Winds Variable and gentle, with fogs in the fore part; in the latter, strong south-west gales, with rain.

Barometer:	Greatest height	30·60 inches.
	Least	29·25
	Mean of the period	30·068
Thermometer:	Greatest height	53°
	Least	22
	Mean of the period	35·86
Mean of the Hygrometer		82
Rain		1·22 inches.
Evaporation, about		0·50 inches.

Tottenham, *First Month*, 26, 1819. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

MARCH, 1819.

ARTICLE I.

Researches on the Measure of Temperatures, and on the Laws of the Communication of Heat. By MM. Dulong and Petit.

(*Continued from p.* 124.)

Of the Dilatation of Solids.

If we compare the results of the preceding table with those that we have given in Table I, it will be seen that the doubts which we raised respecting the rate of the mercurial thermometer were not without foundation; and that the laws of the dilatation of the envelope of this instrument, and of the liquid which it contains, are very distinctly different, when we consider a great interval of temperature. When the air thermometer marks 300° on its scale, mercury taken absolutely would mark 314·15°; while the common thermometer only marks 307·64°.

The preceding determinations are so much the more interesting, because they may lead to the knowledge of the absolute dilatation of several solid bodies. Nothing more is necessary than to ascertain the difference between the expansion of mercury and each of these bodies.

This is easily obtained with respect to glass; for the difference in question is merely the apparent dilatation of mercury in that body. Though this dilatation has been already the object of a great many determinations, we have thought it necessary to undertake it ourselves with all the care that such experiments require. For this purpose, we employed a tube of about 6 decimetres in length, and capable of holding about 700 grammes of mercury.. This tube was shut at one of its extremities, and at the other was terminated by a capillary tube, whose capacity was an insignificant fraction of that of the principal tube.

The whole apparatus being filled with mercury, and carefully freed from air and moisture by repeated boilings, we determined the weight of the mercury which was driven out, when the temperature was raised from freezing to boiling water. We shall be able to appreciate the accuracy of this process, if we remark that the portion of the mass which does not participate in the heating is insensible, and that the horizontal position of the tube allows us, in the case of boiling water, to apply to its temperature the correction depending upon the barometrical pressure.

This experiment, repeated five times on different quantities, furnished for the dilatation sought numbers almost identical, the mean of which is given below. Nor have we found any appreciable difference between the effects observed in tubes of ordinary glass obtained from different manufactories, whatever was their calibre or their thickness.

The values of the apparent dilatation at 200° and at 300°, have been deduced from the preceding comparison made of the scales of the mercurial and air thermometers.

TABLE III.

Temperatures deduced from the dilatation of air.	Mean apparent dilatations of mercury in glass.	Absolute dilatations of glass in volume.	Temperatures deduced from the dilatation of glass (supposed uniform).
100°	$\frac{1}{6480}$	$\frac{1}{38700}$	100·0°
200	$\frac{1}{6378}$	$\frac{1}{36300}$	213·2
300	$\frac{1}{6318}$	$\frac{1}{32900}$	352·9

The first two columns of this table require no explanation. We perceive in them the apparent dilatation of mercury in glass, between 0° and 100°, a little less than that of MM. Lavoisier and Laplace, who appear to have adopted $\frac{1}{6300}$. We expected a difference on the side in which it has taken place; for in the work in which this last determination is stated, the authors have taken care to inform us that they suspected it to be too great, because they did not boil the mercury in the vessel which they employed. The absolute dilatation of mercury which they deduced from it, and which has been generally employed since, ought, therefore, to be too great also, and this is confirmed by the result contained in Table II. The third column gives the dilatations of glass obtained by the method indicated above. This dilatation is increasing; but between 0° and 100°, we find it as stated by Lavoisier and Laplace. The last column contains the degrees which would be indicated by a thermometer formed of a glass plate, whose increase in length would serve as a measure of temperatures. We see by the deviation which has already taken place at 300°, how far the dilatation of glass is from being uniform.

The same process seems capable of being employed to measure the expansion of iron, by enclosing the mercury in an iron vessel. But our attempts in this way not having completely succeeded, we had recourse to the following method. In a glass tube of 18 millimetres in diameter and 6 decimetres in length, and shut at one of its extremities, we introduced a cylindrical rod of iron, which was contained in the axis of the tube by four small traverses of a length, almost equal to the diameter of the tube. After having cemented to the extremity of the tube another capillary tube, we filled it entirely with mercury, which was boiled for a sufficient time to drive out completely the air and humidity. By exposing it then to different temperatures, and determining the weight of the mercury which was driven out, it is easy to deduce the dilatation of the iron; for the volume driven out represents obviously the sum of the dilatations of the mercury and the metal, diminished by the dilatation of the glass. To make the calculation, it is necessary to know the volumes of these three bodies at the temperature of freezing water; but that of the iron is obtained by dividing its weight by its density at zero. We deduce in the same manner the volume of the glass from the quantity of mercury which fills it at zero. That of the mercury is obviously the difference of the first two.

The process which we have just pointed out may be applied likewise to other metals, taking the precaution merely to oxidize their surface, to hinder the dissolving action of the mercury. It is obvious that the coating of oxide is so small that it can produce no change in the result. This method succeeded very well with us for copper, and we regret that we had not time to try the other metals. When we know with precision the expansion of a solid body, we easily deduce from it that of others, by studying the expansion of a metallic pyrometer formed by the union of two rules united in an invariable manner by one of their extremities. In this way we measured, in a new series of experiments, the dilatations of a platinum rule by uniting it to a rule of copper. The dilatation of this last metal already found was verified by uniting a rule of copper to one of glass.

We have collected in the following table the results obtained by these different researches. It contains the mean dilatations of iron, copper, and platinum, taken at first between 0° and 100°, and then between 0° and 300°. We have not given any intermediate determination, because the sole object which we had in view was to assign the degrees in which the different thermometric scales differ from each other. But in order to make the results more evident, we have deduced from each dilatation the temperature which would result if we suppose the expansion of the body uniform. They are the temperatures which thermometers constructed of these bodies would indicate.

TABLE IV.

Temperature deduced from the dilatation of air.	Mean absolute dilatation of iron.	Temp. indicated by a thermometer made of a bar of iron.	Mean absolute dilatation of copper.	Temp. indicated by a thermometer made of a copper rod.	Mean absolute dilatation of platinum.	Temp. indicated by a thermometer made of a platinum rod.
100°	$\frac{1}{28200}$	100·0°	$\frac{1}{19400}$	100·0°	$\frac{1}{37700}$	100·0°
300	$\frac{1}{22700}$	372·6	$\frac{1}{17700}$	328·8	$\frac{1}{36300}$	311·6

When we compare these results with those which we have already obtained for glass, we see that the dilatability of solids, referred to an air thermometer, is increasing; and that it is unequally so in each of them. This consequence, which we have already pointed out in the memoir above quoted, is now, therefore, fully confirmed.

We conceive that we have attained in what precedes the highest degree of accuracy consistent with such delicate measurements. This becomes evident by comparing the numbers which we give for the dilatation at 100°, with those which have been given by Lavoisier and Laplace. We shall add only a single observation. In the direct measurement of the dilatation of solids, the uncertainty is tripled, when we pass from linear expansion to expansion in volume. As our determinations give this last immediately, the error committed is not increased.

Of the Specific Heat of Solids at different Temperatures.

From the results of the preceding researches, we see that if we refer a series of any phenomena whatever to a thermometer taken successively from the gases, the liquids, and the solids, each species of instrument would lead to a particular law. It is not then indifferent to employ any thermometer whatever, if we wish to arrive at the most simple law; or if we wish to represent the phenomena by measures which have from their nature the most direct relations with them. But to be able to determine in this respect, we must know how much the capacities of all bodies vary in each of the thermometric scales which we have made known.

From the time that Black established the notion of capacities, a great number of philosophers have endeavoured to attain greater and greater precision in the numerical determinations relative to each substance, and to include in their tables bodies not hitherto subjected to experiment. The experiments of Wilke, of Crawford, of Meyer, and especially of Lavoisier and Laplace, are, as is known, the most remarkable of all those which have been published on this subject. Deluc and Crawford, supposing an ideal thermometer in which the capacities were constant, compared its indications with those of a mercurial thermometer to judge of the accuracy of this last. Almost all their experiments are referable to mixtures of liquids, below the temperature of boiling water. We see that, reversing the question, this comes to ascertaining whether the capacities of these liquids remain constant when we measure the temperatures by a mercurial thermometer. The results of these two philosophers are different. According to the former, there is a slight variation in the capacity of water in the interval of the first 100°. The second, on the contrary, admits that the capacities are constant. This discordance proves, that within the limits in which the experiments were made, the variation of the capacity of bodies, if it exists, is very small. But such experiments are by far too limited to warrant us to conclude with Crawford, that the same principle extends to all temperatures.

Mr. Dalton, who has considered the question, in his ingenious work that we have already quoted, affirms that the capacity of the same mass of matter does not remain constant, because a part of the heat is expended in producing the dilatation; but that it remains invariable, if we consider the same volume.

This assertion of Dalton is not founded on any direct experiment, and may be considered as a simple conjecture, which is in unison with his other ideas about the measure of temperatures. We shall discuss immediately the principles upon which the whole of his theory rests.

It is obvious that this problem cannot be solved without embracing a much greater extent of thermometric scale than has been hitherto done. Accordingly, the experiments which we are about to give an account of were all made on an interval of 300° or even 350°.

The season of the year in which we have been obliged to devote ourselves to these researches not permitting us to employ conveniently the melting of ice, we have always employed the method of mixtures; but with all the precautions requisite to insure their accuracy.

The bodies whose capacity we have determined, required to be taken from among the metals most difficult of fusion. The homogeneity and the more perfect conductibility of these substances rendered them more proper than any other for the object which we had in view.*

One of the greatest difficulties to which this kind of experiment is subject is the exact determination of the temperatures. We have always employed boiling water to get the capacity below that term; and when the nature of the bodies allowed us to plunge them in boiling mercury, we made use of this second term, as fixed as the first, and which had been determined with the greatest care, as we have before stated.

But when the substance was soluble in mercury, we heated it in an oil bath, which, from the way in which our apparatus was disposed, could preserve a stationary temperature for about a quarter of an hour.

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* Each of the metals got the form of a flat plate, in order to present the more surface. These different plates weighed from one to three kilogrammes.

Finally, to avoid the error which would have been occasioned by the unequal temperature of different points of the mass, we continually agitated the liquid, when at the maximum temperature; and, by means of a thermometer of a constant volume, we had exactly the mean temperature, which must be likewise that of the body. The fixed oils acquiring, as is known, a very great fluidity, when they are very hot; the coat of them which remains attached to the body plunged into them is very thin. However, we did not neglect to take into view the heat coming from this addition of matter, though in most cases the correction was exceedingly small indeed.*

When the body subjected to experiment had been raised to a certain temperature, measured by the means just stated, it was plunged as rapidly as possible into a great mass of water, and the temperature of this liquid was observed as soon as the equilibrium was established. It was in the measurement of this temperature that it was necessary to apply the greatest precautions, in order to obtain results on which dependance could be put. We always employed so great a quantity of water that the increase of temperature never exceeded five or six centigrade degrees. To measure it, a thermometer was employed whose divisions corresponded to the hundredth part of a degree. The water was contained in a very thin vessel of tin plate, placed insulated upon three points. This vessel participated in every case of the heat; but as its weight and its specific heat were accurately known, it was easy in all the calculations to make allowance for it.

In most of the experiments, the water was cooled beforehand such a number of degrees, that, after the immersion of the body, it was raised to the temperature of the surrounding atmosphere. In other cases, it was of the temperature of the air before the experiment. The first method appeared to us most accurate, and it did not require any correction; for the water, immediately after the body is plunged into it, acquiring a temperature very little different from that which it has when the equilibrium is established, the external air must produce only an imperceptible effect on it. In the second method, on the contrary, we must take into the account the loss of heat which the mass experiences in consequence of its excess of temperature and the duration of the experiment. This correction may be determined with a sufficient degree of precision by a subsequent observation on the rate of cooling of the water employed. The large size of the body which we employed, the varied circumstances in which each determination was made, the precision of the thermometer which we employed, seemed all to concur to ensure the accuracy of our results.

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* This correction is deduced from the weight of oil, carried off by the metallic plate. To ascertain it, we were obliged in each case to make a preliminary experiment, in which we ascertained the increase of the weight of the plate when it came out of the oil bath. At 300° this increase never exceeded three or four decigrammes.

The great capacity of iron (relatively to the other metals), and the possibility of plunging it in boiling mercury, induced us to begin with this substance, the comparisons which we proposed to make. The following determinations are deduced from a great number of measurements, agreeing nearly with each other.

Mean capacity of iron from 0° to 100° = 0·1098*
0 to 200 = 0·1150
0 to 300 = 0·1218
0 to 350 = 0·1255

The result, indicated by the way in which these numbers vary, is verified in the following table for the other metals. We have satisfied ourselves with inserting the measures taken at 100° and at 300°.

	Mean capacity between 0° and 100°.	Mean capacity between 0° and 300°.
Mercury	0·0330	0·0350
Zinc	0·0927	0·1015
Antimony	0·0507	0·0549
Silver	0·0557	0·0611
Copper	0·0949	0·1013
Platinum	0·0355	0·0355
Glass	0·1770	0·1900

The capacity of solid bodies then follows the same law with their liquidity; it increases with the temperatures, measured by an air thermometer. They would even be increasing, contrary to the opinion of Crawford, if we were to employ a mercurial thermometer. If this observation had been made upon bodies of an invariable volume, there would remain no doubt respecting its consequences; but the gaseous is the only state which permits us to satisfy this condition; and in that state the experiment presents insurmountable difficulties. If the dilatation of solids were uniform, we could not ascribe the increase of capacities to the quantity of heat which produces the increase of volume; for this quantity remaining then proportional to the temperatures, could not affect the ratio of the capacities. The case is not the same when the dilatabilities are increasing. It is evident that in this case the capacities taken at different heights of the thermometrical scale ought to be affected by the irregularity of the law of dilatation. We cannot form any conjecture of the intensity of the effects due to this accidental cause. But what would lead to the belief that they should not be neglected, and that the increase of the capacities which we have observed, depends, at least in part, upon them, is this, that the metals whose expansion follows the most rapid law, are at the same time those whose capacity undergoes the greatest variations. But this question can only be decided by observations, which should embrace a greater interval of temperature than that which existed in our experiments. We hope soon to be able to throw light on this doubtful question.

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* The capacity of water is reckoned 1.

We have shown, in speaking of the dilatation of solids, that if we construct thermometers with the most infusible metals, and suppose them graduated as usual by the fixed points of freezing and boiling water, the temperatures indicated by each of these thermometers would be very different. The same discordance ought evidently to be observed from what precedes, when we estimate the temperatures, as some philosophers have proposed, by the ratios of the quantities of heat which the same body gives out in cooling to a determinate temperature. In fact, in order that this calculation should be exact, it would be necessary that the body in cooling, for example, from 300° to 0° should give out three times as much heat as in cooling from 100° to 0°. But it will give out more than three times as much, because the capacities are increasing. We should, therefore, find too high a temperature. We exhibit in the following table the temperatures that would be deduced by employing the different metals contained in the preceding table. We must suppose that they have been all placed in the same liquid bath at 300°, measured by an air thermometer.

Iron	332·2°
Mercury	318·2
Zinc	328·5
Antimony	324·8
Silver	329·3
Copper	320·0
Platinum	317·9
Glass	322·1

General Reflections and Conclusion.

Now that we have established the correspondence of all the thermometric scales in an extent of more than 300°, we are in a state to judge of the accuracy of the laws which Mr. Dalton conceives he has established, by measuring the temperatures upon a particular scale.

The way in which Mr. Dalton has presented the principle on which the formation of his scale depends, does not allow us to consider it as any thing else than an hypothesis, which would have the advantage of connecting together a great number of phenomena by very simple relations. By substituting the indications of that scale for those of the ordinary thermometer, we find, according to this philosopher:

1. That mercury and all other liquids dilate as the squares of their temperatures, setting out from the maximum density of each.

2. That the gases dilate in a geometrical progression for increments of temperature in an arithmetical progression.

3. That the capacity of bodies remains constant under the same volume.

4. That during the whole time of the cooling of bodies in air, the temperatures decrease in a geometrical proportion, while the times follow an arithmetical progression.

All these laws are very accurately verified in Mr. Dalton's thermometer for the temperatures near those in which the new scale coincides with the old; and if the same agreement were observed at all temperatures, the union of these laws would undoubtedly form one of the most important acquisitions of modern physics. But unfortunately this agreement is very far from taking place in very low or very high temperatures, as we shall now show.

In setting out from the first two laws, we should find, by a very simple calculation, that a volume of air, represented by 1000, at the temperature of freezing water, would be reduced to 692 at the point of the congelation of mercury. We found by our experiments, that its volume would be 850. At the temperature of 256° on the air thermometer, the common mercurial thermometer ought to mark 282° according to Mr. Dalton, while in reality it only indicates 261°.

Such differences cannot be ascribed to errors of observation. They would be much greater if we went to higher temperatures. It is easy to see that by applying to our determinations the principles of Mr. Dalton, we should find by the absolute dilatations of mercury, that what he calls the real temperature would be much superior to that indicated by the common thermometer. But this would produce an effect precisely contrary to what Mr. Dalton had in view, which was to lower the indications of this last instrument in high temperatures.

The third law does not appear better founded; for we have shown that the capacity increases about a tenth in several bodies whose volume does not vary one hundredth part. And if we estimated the capacities by the scale of which we have just spoken, this law would deviate still further from the truth.

In fine, to prove, in a few words, that the fourth proposition of Mr. Dalton is likewise contradicted by experience, it is sufficient to say, that the law of cooling in the air is not the same for all bodies; and that, therefore, no thermometric scale can satisfy the condition of rendering for all bodies the loss of heat proportional to the excess of temperature.

Though the propositions which we have just discussed do not attain the object which Mr. Dalton had in view, they prove at least that long ago the insufficiency of the common doctrine had not escaped the penetration of this celebrated philosopher. Most of the phenomena, whose irregularity he had perceived, vary in the way which he had pointed out; but he wanted the data necessary to verify his ingenious theory. The researches, of which we have given an account, enable us to present much more certain notions on the measurement of temperatures, and to explain several difficulties which had been started on the subject. It is evident, by what we have said respecting the variation of the capacities, that no thermometric scale can indicate immediately the increments of heat corresponding to a determinate elevation of temperature; for supposing we found one which possessed that property relatively to a peculiar substance, it could not be applied to others, because the capacities of all bodies do not vary in the same way.

By comparing together all the thermometric scales, we may make ourselves equally certain that no one exists in which the dilatations of all bodies can be expressed by simple laws. These laws would vary according to the scale which we adopted. Thus, if we take the air thermometer, the law of the dilatation of all bodies would be increasing. If we chose iron for our thermometer, all bodies would follow a decreasing law of dilatation. If we took the mercurial thermometer, corrected from the complication occasioned by its envelope, iron and copper would follow an increasing law of dilatation, while platinum and the gases would follow one continually decreasing.

In the state to which the question is now reduced, we cannot allege any peremptory reason for adopting one of these scales exclusively. We may say, however, that the well-known uniformity in the principal physical properties of all the gases, and particularly the perfect identity in the laws of their dilatation, renders it very probable, that in this class of bodies the disturbing causes have not the same influence as in solids and liquids, and that consequently the changes in volume produced by the action of heat upon them are more immediately dependant upon the force which produces them. It is, therefore, very probable, that the greatest number of the phenomena relating to heat will present themselves under a more simple form, if we measure the temperatures by an air thermometer. It is at least by these considerations that we have been determined to employ this scale constantly in all the researches which constitute the object of the second part of this memoir. The success which we have obtained may be stated as an additional motive in favour of the opinion which we have given. But we do not pretend that the other scales ought to be excluded in all circumstances. It is possible, for example, that certain phenomena may present themselves in a more simple manner, by reckoning the temperatures on the thermometrical scales deduced from the dilatation of each of the bodies whose dilatations were observed. It was this indeed which led us to follow with so much perseverance the comparisons of all the thermometrical scales.

(*Note added.*)—When treating of the dilatation of mercury, we presented a table of the results obtained by different philosophers on this important subject. The one which has been used in France for several years, and which is ascribed to Lavoisier and Laplace, is found among them. We perceived that it did not agree with the number which Lavoisier gives in his memoir, i. 310, for the apparent dilatation of mercury in glass; but we thought that it was the result of a subsequent and unpublished set of experiments. Since drawing up our memoir, we have learned that these illustrious philosophers did not undertake new experiments on the subject; but that an error had crept into the calculation of the observations; so that the true coefficient deduced from their measure is $\frac{1}{5548}$ instead of $\frac{1}{5412}$. The one which we found by quite a different process, $\frac{1}{5550}$, differs very little from theirs. This is a new proof of the accuracy of our observations.

Part II.—*Of the Laws of Cooling.*

The first views relative to the laws of the communication of heat are to be found in the Opuscula of Newton.* This great philosopher admits, *à priori*, that a heated body exposed to a constant cooling cause, such as the uniform action of a current of air, ought to lose at each instant a quantity of heat proportional to the excess of its temperature above that of the ambient air; and that consequently its losses of heat in equal and successive intervals of time, ought to form a decreasing geometrical progression. Kraft, and after him Richmann,† endeavoured to verify this law by direct experiments on the cooling of liquid masses. These experiments, afterwards repeated by different philosophers, prove, that for differences of temperature not exceeding 40 or 50 degrees, the law of geometrical progression represents pretty exactly the rate of cooling of bodies.

In a dissertation, little known, on several points of the theory of heat, published in 1740, and of course several years before Kraft and Richmann made known their researches, Martine‡ had already pointed out the inaccuracy of the preceding law, and had endeavoured to substitute for it another, in which the loss of heat increased more rapidly than by the Newtonian law.

Erxleben § proved equally, by very accurate observations, that the deviation of the supposed law increases more and more as we consider greater differences of temperatures; and concludes, that we should fall into very great errors if we extended the law much beyond the temperature at which it has been verified. This very just remark of Erxleben, as well as his memoir, seems to have escaped the attention of philosophers ; for in all posterior remarks on the same object, the law of Newton has been presented not as an approximation but as a rigorous and constant truth.

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* Newtoni Opuscula, ii. 425.

† Nov. Com. Ac. Pet. i. 195.

‡ Essays on Heat, p. 72.

§ Novi Comment. Soc. Gotting. viii. 74.

Thus Mr. Leslie,* in his ingenious researches on heat, has made this law the base of several determinations, which, from that very cause, are inaccurate, as we shall prove in the sequel.

Soon after the publication of Mr. Leslie's book, Mr. Dalton made known, in his New System of Chemical Philosophy, a series of experiments on the cooling of bodies carried to a very high temperature. The result of these experiments shows evidently that the law of Richmann is only an approximation at low temperatures, and that it is quite inaccurate at high temperatures. Mr. Dalton, instead of seeking to represent his observations by a new law, endeavoured to re-establish the law of Richmann by substituting for the usual thermometric scale the one which he founded on the notion that the dilatation of all liquids is subjected to the same law ; an assertion which we have discussed in the first part of this memoir. But even supposing the accuracy of the principles of this new scale to have been constated, we should be under the necessity of acknowledging, that it does not satisfy the condition of rendering the loss of heat in a body proportional to the excess of its temperature above that of the surrounding air ; or in other words, that it does not re-establish the law of Richmann; for before this could happen, it would be necessary that the law of cooling should be the same for all bodies, and our experiments prove the contrary.

The last experiments undertaken on the subject which occupies our attention, are those which Laroche has inserted in his memoir, relative to some properties of radiating heat. He establishes, among other propositions, that the *quantity of heat which a hot body gives off in a given time by way of radiation to a cold body situated at a distance, increases, other things being equal, in a progression more rapid than the excess of the temperature of the first above that of the second.*

This proposition is evidently for radiation the equivalent of that of Mr. Dalton for the total cooling of a body in the air. But Laroche has only presented insulated facts, and has not sought for the law on which they depend. We shall see hereafter that the results are complicated by the action of particular causes, from which it would be necessary to disengage them in order to arrive at the law of cooling in a vacuum, which is not the same as radiation.

Thus the labours of philosophers on the laws of cooling have been hitherto confined to showing that the law of Newton is a sufficient approximation at low temperatures; but that it deviates further and further from the truth as the difference between the temperatures increases.

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* An Inquiry, &c.

If in the concise history of these labours we have not mentioned the mathematical researches of M. Fourier, on the laws of the distribution of heat, the reason is, that all the results of his analysis are deduced from the law of Newton, admitted as a truth founded on observation, while the sole object of our experiments is to discover the law that ought to be substituted for it. But the very remarkable consequences to which this profound mathematician has been led, will preserve all their precision in the circumstances and within the limits in which the Newtonian law is true; and to extend them to other cases, it will be sufficient to modify them conformably to the new laws which we shall establish.

Of Cooling in general.

When a body cools in a vacuum, its heat is entirely dissipated by radiation. When it is placed in air, or in any other fluid, its cooling becomes more rapid, the heat carried off by the fluid being in that case added to that which is dissipated by radiation. It is natural, therefore, to distinguish these two effects; and as they are subject in all probability to different laws, they ought to be studied separately. We shall examine then successively the laws of cooling in a vacuum and in elastic fluids. But as the plan which we have followed in each of these researches is founded on the same principles, it will be proper to explain these principles in the first place.

The most simple case of cooling will be that of a body of so small a size that we may suppose at every instant all its points at the same temperature. But to arrive at the object which we proposed, the discovery of the elementary law of cooling, it would have been to add an useless complication to the question, and would have rendered it almost insoluble to have observed, in the first place, the rate of cooling in solid bodies; because in that case the phenomenon includes an additional element, namely, the interior distribution of the heat, which is a function of the conductibility. Obliged by the nature of the problem to have recourse to liquids, the mercurial thermometer itself appeared to us the body best adapted for these experiments. But as it is necessary to be able to observe at high temperatures to give to the body on which the experiment is made such a size that the cooling shall not be too rapid for following its rate with accuracy, it was necessary, in the first place, to examine what influence the greater or smaller mass of liquid contained in the bulb of the thermometer had upon the law of cooling. It was not less important to examine whether that law depends on the nature of the liquid, or the nature or form of the vessel in which it is contained. These first comparisons were the object of a series of experiments, which we shall state, after having explained the uniform mode of calculation which we always employed, in order to render our results more easily comparable.

Suppose we observe, at equal intervals of time, every minute for example, the excess of temperature of a body above the surrounding medium, and that for the times 0, 1′, 2′, 3′, &c. ....*t*′, the excesses are A, B, C. ....T. If the law of geometrical progression held good, we should have B = A m, C = A m^2 ....T = A m^t; m being a fraction which will vary from one body to another. This law never holds exactly, especially when the temperatures, A, B, C, are high. But it is clear that we may always represent a certain number of the terms by an expression of the form A $m^{\alpha t + \beta t^2}$, by determining properly the coefficients m, α, β; and by means of that formula, we may calculate very nearly the value of the time t, corresponding to any excess of temperature T, provided that this excess be comprehended in the portion of the series which has served for the interpolation.

This same expression gives us the means of determining the rapidity of cooling corresponding to each excess of temperature; that is to say, the number of degrees which the temperature of a body would sink in a minute, supposing the rate of cooling uniform during that minute. We have in fact for that velocity,

$$\frac{dT}{dt} = (\log. m) . T . (\alpha + 2 \beta t)$$

This quantity must always exceed the real loss of temperature during the time, since the rapidity of cooling diminishes during its whole duration, how short soever it may be.

It was not, as may be easily conceived, to correct the small difference of which we have just spoken that we employed this process. But it is obvious that when a series is divided into several parts, represented each by empirical formulas, which correspond as exactly as possible with the numbers observed, the velocity of cooling deduced from these formulas for the different excesses of temperature, are always disengaged from the uncertainties and inaccuracies which the crude results of the observations always present.

Let us return now to the first comparison, of which we spoke a little ago; and for this, let us examine how the velocity of cooling has varied in the three series, the calculated results of which are contained in the following table:

Excess of temperature above the air.	Velocity of cooling of the thermometer A.	Ditto of thermometer B.	Ditto of thermometer C.
100°	18·92°	8·97°	5·00°
80	14·00	6·60	3·67
60	9·58	4·56	2·52
40	5·93	2·80	1·56
20	2·75	1·30	0·73

The first column contains the excess of the temperature of the thermometers above that of the surrounding air. The second exhibits the corresponding velocities of cooling of the thermometer A, the diameter of whose bulb was about two centimetres. These velocities were calculated from the observations by the method explained above. The third and fourth columns exhibit the velocities of the cooling of the thermometers B and C, calculated in the same way for the excess of temperatures indicated in the first column. The diameter of the bulb of the thermometer B was about four centimetres; that of the thermometer C about seven.

A simple inspection of this table shows us at once the inaccuracy of the law of Richmann; for we see that the velocities of cooling increase according to a more rapid progression than the excesses of temperature. Now if we take the ratios of the corresponding numbers in the second and third columns, we shall find that they have varied as follows, beginning with the terms which correspond with the greatest excess of temperature:

2·11 ...... 2·12 ...... 2·10 ...... 2·12 ...... 2·11

These numbers, which differ very little from each other, and which are alternately greater and less, inform us that the rate of cooling follows the same law in the two thermometers A and B. If we compare in the same way the numbers contained in the second and fourth columns, we obtain for their ratios:

3·78 ...... 3·81 ...... 3·80 ...... 3·80 ...... 3·77

The near approximation to equality in these numbers shows us that the law of cooling is likewise the same for the thermometers A and C; for the differences in the preceding numbers must be ascribed to unavoidable errors in the experiments; and they are owing to inaccuracies merely of one hundredth of a degree in the velocities.

We are entitled to conclude, from what precedes, that the law of cooling, observed in a mercurial thermometer, is independent of the size of its bulb, and of consequence that it is the elementary law of cooling, of which we are in search; or, in some measure, the law of cooling of a point.

We have not examined how the velocity of cooling varies with the extent of surface, in consequence of the little precision of measurement of which the surface of a ball of glass blown at the extremity of a tube is susceptible; and because that research was foreign to the object which we had in view. However, it will be seen from the approximate measures which we have given of the diameters of the bulbs, that the velocities of cooling are nearly inversely as the diameters, as would be the case with a solid sphere of infinitely small size.

Let us now proceed to the examination of the influence which the nature of the liquid in the vessel may have upon the law of cooling. There the difficulty of constructing thermometers with liquids different from mercury—a difficulty depending upon the uncertainty, which still exists, respecting the laws of the dilatation of these bodies, determined us to observe the cooling of these liquids enclosed in the same glass matrass, in the centre of which was placed a very sensible mercurial thermometer. We even ascertained that the position of the thermometer is indifferent, and that at any given instant the temperature of all the points of the mass is sensibly the same. This evidently depends upon the interior conductibility, which, in liquids, is the result of currents, being nearly perfect, at least for masses of the size of those which we employed.

The first of the following tables contains the velocities of the cooling of mercury and water compared; the second exhibits a similar comparison between mercury and absolute alcohol; and the third, between mercury and concentrated sulphuric acid.

Excess of the temperature of the body.	Velocity of cooling of mercury.	Ditto of water.	Ratio of these velocities.
60°	3·03°	1·39°	0·458
50	2·47	1·13	0·452
40	1·89	0·85	0·450
30	1·36	0·62	0·456

Excess of the temperature of the body.	Velocity of cooling of mercury.	Ditto of absolute alcohol.	Ratio of these velocities.
60°	1·89°	1·50°	0·798
30	1·36	1·09	0·801
20	0·87	0·69	0·794

Excess of the temperature of the body.	Velocity of cooling of mercury.	Ditto of sulphuric acid.	Ratio of these velocities.
60°	3·03°	1·97	0·650
50	2·47	1·59	0·649
40	1·89	1·22	0·646
30	1·36	0·89	0·654

The ratios, inserted in the last columns of these tables, show us that the law of cooling is the same for the four liquids compared; for the small irregularities in these ratios proceed evidently from uncertainties in the observations; and besides, to make them disappear, it would be sufficient to alter the values of the veloci-

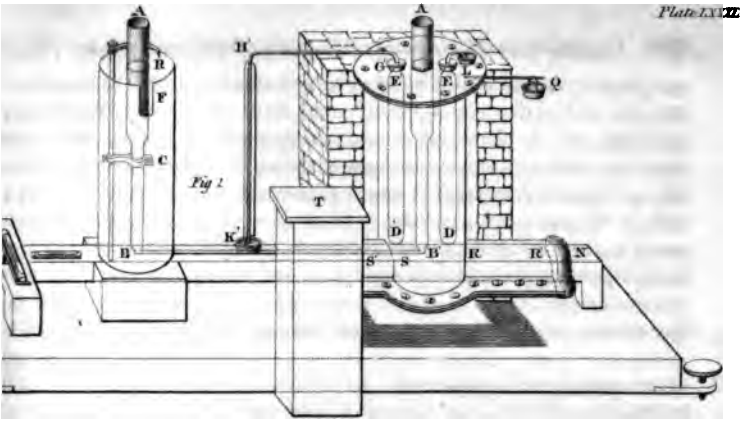
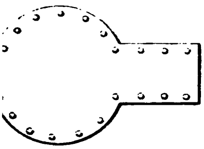
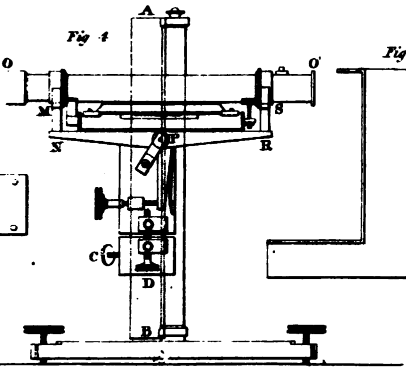


Fig 1

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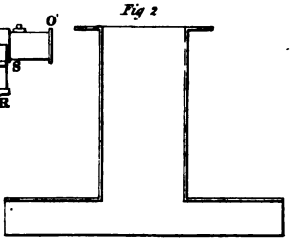


Fig 2

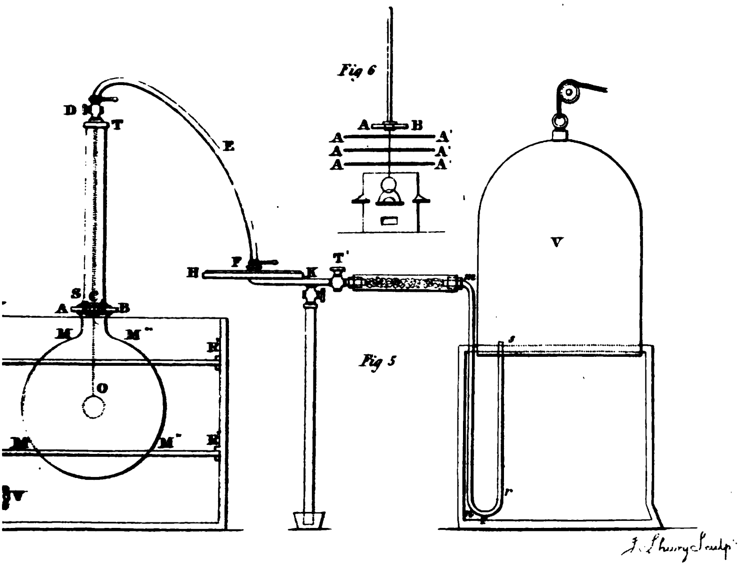


Fig 6

Fig 5

J. Shury Sculp.

Engraved for Dr. Thomson's Annals — for Baldwin Cradock & Joy, Paternoster Row, Mar. 1. 1819.

ties observed by quantities, which scarcely amount to the hundredth of a degree.

Now if liquids so different in their nature, their density, and their fluidity, exhibit laws of cooling absolutely similar, is it not natural to draw the same consequence to which we were already led by a comparison of the cooling of unequal masses—That within the limits of our observations, the cooling of a liquid mass is subjected to the same law as a body of infinitely small dimensions?

It remains now to examine the influence of the nature and shape of the vessel.

We, in the first place, compared the cooling of two spheres; the one of glass, the other of tin plate, both filled with water. (The diameter of the tin plate sphere was a little greater than that of the glass sphere.)

Excess of the temperature of the body.	Velocity of cooling of the glass sphere.	Ditto of the tin plate sphere.	Ratio of these velocities.
60°	1·39°	0·90°	1·54
50	1·13	0·73	1·55
40	0·85	0·54	1·57
30	0·62	0·38	1·63
20	0·37	0·21	1·76

Here the ratios in the fourth column vary always the same way, and show us that the law of cooling is more rapid in the tin plate sphere than the glass sphere. Mr. Leslie obtained the same result, which he has generalized by admitting that this law changes with the nature of the body, and that it is most rapid in those bodies that radiate least. This proposition is true in the portion of the scale to which Mr. Leslie's experiments were confined; but, by a very remarkable casualty, the contrary effect takes place at high temperatures; so that when we compare the laws of cooling of two bodies with different surfaces, that of the two laws which is *most* rapid at the lower part of the scale, becomes the least rapid at high temperatures. Thus in the series given above, the ratios, inserted in the last column, diminish in proportion as we consider greater excesses of temperature; they should increase; and as is the case with all quantities which change their sign, these ratios remain nearly the same during a considerable extent of the thermometric scale. This is one of the most important points of the theory of cooling. If we do not deceive ourselves respecting the accuracy of our observations, a very simple explanation will be found in the subsequent part of this memoir of this remarkable fact, which can only be observed by making experiments, as we have done, on the cooling of bodies raised to a high temperature.

It is because they did not follow this plan that Messrs. Dalton and Leslie have obtained such inaccurate results respecting this question. The first, led away without doubt by the notion that the law of Richmann is verified in his thermometric scale, and not having compared the cooling of different surfaces for a sufficiently large interval, had been led to suppose that the law of cooling is the same in all bodies. And Mr. Leslie, who had remarked that the law changes with the nature of the surface, not having included in his experiments temperatures sufficiently high, concluded, that the difference which he observed always increases as we advance in the thermometric scale. This has led him to consequences very far from the truth, respecting which we shall have occasion to make observations in the sequel. We shall merely remark, in expressing our surprise, that Mr. Leslie, whom the influence of the nature of the body on the law of cooling did not escape, and who had concluded in consequence that the law of Richmann must be inaccurate, has nevertheless made use of this in most of his experiments.

We terminated these preliminary researches by examining the cooling of water in three vessels of tin plate of the same size; the first spherical; the second cylindrical, having a height equal to twice the diameter of its base; and the third likewise cylindrical, but having a height equal to half its diameter.

Excesses of temperature.	Velocity of cooling of the sphere.	Ditto of the first cylinder.	Ditto of the second cylinder.	Ratio of column three to column two.	Ratio of column four to column two.
60°	0·90	1·11	1·01	1·23	1·12
50	0·73	0·89	0·80	1·22	1·10
40	0·54	0·66	0·60	1·22	1·11
30	0·38	0·47	0·43	1·23	1·13
20	0·21	0·26	0·23	1·24	1·10

The law of cooling is still the same for the three vessels of different shapes, as appears by the ratios contained in the last two columns. The form of the vessel then has no influence on the law of cooling; and this assertion is confirmed by this, that the ratios found between the velocities of cooling are nearly the same as those that exist between the surfaces of the vessels, as may be easily ascertained. On recapitulating the results which we have just made known, we see that the law of cooling of a liquid mass, though it varies with the enveloping surface, is nevertheless independent of the nature of the liquid, and of the form and size of the vessel which contains it. This was the point which we proposed to establish in this introduction, and which constituted the basis of the researches which we are now going to explain.

Apparatus destined for Experiments on Cooling.

The bodies, whose cooling we observed, were, conformably to the principles just explained, thermometers of such a size that the diminution of their temperatures could be observed with precision. We constructed two of them, the bulb of one of which had a diameter of about six centimetres; that of the other of two. The first, containing about 3 lb. of mercury, served for observations at a high temperature. The smaller one was employed for low temperatures, in order to shorten the duration of the experiments. It was easy to deduce from the results given by the last, those which would have been given by the large one if the series of its cooling had been prolonged; for that, it was sufficient to commence the observations with the small thermometer at a higher temperature than that at which the large one had terminated. By determining then the ratio of the velocity of the cooling of this last to that of the small thermometer for a common excess of temperature, we obtained the number by which it was necessary to multiply all the results given by the small thermometer to obtain the corresponding velocities of the other.

These two instruments, constructed with all the care possible, did not differ from common thermometers, except in this particular, that the tube on which the degrees were marked was separated from the bulb by an intermediate tube, the calibre of which was very small. We shall see immediately the motive of this construction.

The experiments on cooling in a vacuum, with which we had to commence, required that the thermometer could be transported into a pretty large space, in which a vacuum could be made very rapidly. It was necessary also that the surface which surrounded the thermometer on every side should be maintained at a known temperature: and as it was requisite that the same apparatus should serve for observing the cooling in air and in gases, it was requisite that the gases should be introduced into it in a convenient and prompt manner. All these conditions were satisfied by the following construction.

The enclosure in which the cooling takes place is formed of a large, thin, copper balloon M M′ M″ M‴ (fig. 5) whose diameter is about three decimetres. The neck of this balloon is ground at its upper part so as to be terminated by a perfectly flat surface, which is rendered horizontal by means of a level. This balloon is plunged almost completely into a large wooden cylindrical trough full of water, in which position it is kept by the strong cross beams, R R′, R R′. It is evident that the walls of this balloon, being very thin and very good conductors, must assume constantly the temperature of the surrounding water; and being covered within with lamp-black, they cannot reflect any sensible quantity of the heat sent to them from the thermometer. Besides, this effect, if it were to take place, would increase almost as the loss of heat of the body, so that the error produced would affect equally all the results. It was easy to raise the temperature of the surrounding medium, by passing vapour into the water through the tube S′ U V, plunging to the bottom of the liquid.

The orifice of the balloon is shut by a thick plate of glass, A B, ground with the greatest care upon the edge of the balloon itself. The surfaces in contact have besides, in consequence of the thickness of the neck, a sufficient extent, so that the interposition of a small quantity of hog's lard renders the contact very close, and prevents all communication from without.

This plate is perforated at its centre by a circular opening, into which a cork is firmly put, which contains the tube of the thermometer; and the intermediate tube, C O, is of such a length that the bulb is precisely in the centre of the balloon. By giving this intermediate tube a very small diameter, the quantity of mercury without the bulb is diminished, and the swelling which takes place at the commencement of the scale enables us to fix the tube more firmly in the cork. Thus the thermometer is fixed in the plate, and this disposition is shown particularly in fig. 6, where the bulb of the instrument is placed above the furnace, which serves to heat it. The screens, A A′, are leaves of tin plate, separated from each other, which serve to screen the plate, A B, from the action of the heat.

Let us now return to fig. 5. The stem of the thermometer, which is without the balloon, as is evident from the figure, is covered by a hollow tube, S T, the ground bottom of which is applied to the upper surface of the glass plate. This kind of vessel is terminated above by a stop-cock, to which is cemented the end of the very flexible leaden tube, D E F. The other extremity of this tube is firmly fixed to the plate of an air-pump, H K. The canal, which in this machine makes the communication between the centre of the plate and the barometer, is connected with another tube with a stop-cock, to which is cemented a tube filled with muriate of lime. It is through this tube that the gas passes by way of the bent tube, *m n p r s*. The glass air holder, being moveable up and down, enables us to make the elasticity of the gas introduced the same as that of the atmosphere. We shall now describe our mode of proceeding in each experiment.

The water in the trough being brought to the requisite temperature, and the thermometer fixed in the glass plate, being heated to nearly the boiling point of mercury, it was transported rapidly into the balloon. The glass, S T, already cemented to the leaden tube, was then drawn down over the stem. While the surfaces in contact were carefully luted, an assistant rapidly exhausted the balloon, by means of the air-pump. The communication between the balloon and the glass tube was rendered very free by the openings, *a* and *b*, made in the plate near the central opening.

If the cooling was to be observed, in vacuo, the process was stopped when the machine ceased to dilate the air, and we measured immediately the tension of what remained in the balloon. The stop-cock was then shut, and the observations commenced. When the experiment was to be conducted in air, that of the balloon was at first dilated, in order to facilitate the contact of surfaces, and then the proper quantity was allowed to enter. When the cooling was to be observed in a gas, the balloon was first emptied of air, gas was then allowed to enter, and a vacuum was again made, after which the requisite quantity of gas was introduced. By this contrivance, it was mixed with only an inappreciable quantity of air.

We shall terminate this description by saying, that the dimensions of the thermometer had been calculated, so that the observation of the cooling could begin at about 300°. The experiments in air and in the gases require rather a longer preparation, and cannot be commenced with safety till the equilibrium is restored through the whole extent of fluid. The series of observations belonging to them commence at about 250°.

The experiment for cooling in vacuo, or in gases, being thus prepared, it remained merely to observe the rate of cooling by means of a watch with a second's hand at equal intervals of time. But these temperatures require two corrections, which we shall point out. In the first place, it is obvious, from the nature of our apparatus, that after a short time the stem of the thermometer was cooled down to the temperature of the surrounding air. Every temperature observed, therefore, was too low, by a number of degrees equal to that to which the mercury in the stem would dilate, when heated from the temperature of the surrounding atmosphere to that of the bulb. This correction was easily calculated, and was applied to all the temperatures observed. The object of the second correction was to reduce the indications of the mercurial thermometer to that of the air thermometer. For this we employed the table given in the first part of this memoir.

Having thus obtained a series of consecutive temperatures of the thermometer, it only remained to apply to that series the mode of calculation which we have explained above. We divided it then into two parts, which were represented each by expressions of the form $m \,.\, a^{\alpha t + \beta t^2}$ in which t denotes the time; and these formulas served to calculate the velocity of cooling for the different excesses of temperature; but these velocities required a diminution easily determined in each case. That it may be conceived in what this consists, we must remark, that the cooling of the bulb of the thermometer, arising from the loss of heat which takes place at the surface, is always a little augmented by the entrance of cold mercury from the stem of the thermometer. But the volume of mercury being known, and likewise its temperature, it was easy to estimate exactly the amount of this correction, which, though very small, ought not to have been neglected.

Such is the mode which we always followed, in conducting and calculating all our experiments. We satisfied ourselves with determining the velocity of cooling for excesses of temperature, differing from each other by 20 degrees. And that we might not make this memoir too tedious, we have withheld all the intermediate calculations which led to our determinations.

We shall now enter upon a detail of our experiments, stating them in the order in which they were made.

Our preliminary researches having made us acquainted with the influence of the nature of the surface upon the law of cooling, it was necessary to study that law under different states of the surface of our thermometers. But it was necessary likewise that these surfaces should not experience any alteration from the highest temperatures to which they should be exposed. The only two which appeared to us to answer this condition are surfaces of glass and of silver. Accordingly most of our experiments were made, first preserving to the thermometer its natural surface, and then covering it with a very thin leaf of silver. These two surfaces possess, as is known, very different radiating powers; glass being one of the bodies which radiate most, and silver of those which radiate least. The laws to which we have arrived, by comparing the cooling of these two surfaces, are of such simplicity that there can be no doubt of their being applicable to all other bodies.

(To be continued.)

ARTICLE II.

On Mr. Tennent's Bleaching Salt; known by the Name of Oxymuriate of Lime. By Thomas Thomson, M.D. F.R.S.

WHEN I was drawing up the fifth edition of my System of Chemistry, one of the substances, respecting which I found myself unable to form a definite opinion, was the bleaching salt, originally invented by Charles Tennent, Esq. and well known in commerce under the name of *oxymuriate of lime.* I found myself, therefore, obliged to omit the substance altogether, resolving, however, to ascertain its nature as soon as I should have sufficient leisure for that purpose. I got a quantity of it accordingly from Mr. Tennent, last autumn, quite fresh, and subjected it to the requisite experiments. I shall in this paper give a short sketch of the principal facts which I observed, reserving a more detailed account of the experiments till some future opportunity; for more time than I can at present command would be requisite to disentangle the useful from the immaterial or indecisive experiments, with which they are mixed in the journal which was written down at the time.

1. *Oxymuriate of lime,* when recently prepared, is quite dry to the feel. It has a peculiar smell, bearing some relation to that of chlorine, but not so offensive. Its taste is hot and astringent. The hot taste is probably owing to the uncombined quick-lime contained in the powder; for when the oxymuriate of lime is dissolved in water, the taste of the solution is merely astringent.

2. Fifty grains of the powder being digested in a sufficient quantity of water to dissolve the soluble part of the salt, and poured upon a filter, left a quantity of lime, partly in the state of quick-lime, and partly in the state of carbonate. Concluding that the carbonic acid had been absorbed during the drying of the lime, I digested the insoluble residue from other 50 gr. of the salt in diluted sulphuric acid, evaporated the liquid to dryness, and exposed the sulphate of lime, thus obtained, to a red heat. It weighed 27·8 gr. indicating 11·68 gr. of lime.

3. The portion of the oxymuriate of lime dissolved in water reddened turmeric paper, and when left exposed to the air, a crust of carbonate of lime was formed in the surface of the liquid. Hence it was obvious, that besides the bleaching salt, or oxymuriate of lime, the water had dissolved likewise a portion of lime, and was, therefore, in the state of lime water.

4. The solution obtained by digesting 50 gr. of the bleaching powder in water and filtering, was mixed with an excess of sulphuric acid, evaporated to dryness, and exposed to a red heat. The sulphate of lime formed weighed 31·5 gr. indicating 13·23 gr. of lime.

5. Another similar solution, obtained from 50 gr. of the bleaching powder, was precipitated by nitrate of silver. The chloride of silver obtained weighed 55 gr. indicating 13·56 gr. of chlorine.

6. Now if we consider the bleaching salt to be a combination of chlorine and lime, or a chloride of lime, as I shall afterwards show it to be, we shall find the quantity of uncombined lime in the solution by subtracting from the total quantity of dissolved lime that portion of it which is in combination with the chlorine. This quantity may be found as follows:

The weight of an atom of chlorine is 4·5, and of an atom of lime 3·625; the quantity of chlorine in 50 gr. of the bleaching powder is 13·56 gr.; therefore, 4·5 : 3·625 :: 13·56 : 10·92 = lime united to the chlorine.

Total lime in the solution.	13·23
Lime united to the chlorine	10·92
Uncombined lime in the solution.. =	2·31

7. From the preceding experiments, it follows (supposing what is called oxymuriate of lime to be a chloride of lime), that the composition of the bleaching powder which I examined was as follows:

1. Undissolved portion; viz. lime		11·68
2. Dissolved portion:	1. Chlorine	13·56
	2. Combined lime	10·92
	3. Uncombined lime	2·31
		38·47

Hence the water and the impurities in 50 gr. of the bleaching powder, which I examined, amounted to 11·53 gr. or somewhat more than $\frac{1}{5}$th of the total weight.

8. The uncombined lime in the specimen which I examined, which was newly prepared, amounted to 15·87 gr.; while the portion united to the chlorine was 10·92 gr.; so that the uncombined lime was to the combined nearly in the proportion of three to two. In the experiments which Mr. Dalton published on this salt, in the first number of the *Annals of Philosophy*, he found one half the lime united to the chlorine. I made some attempts to repeat his mode of analysis, but did not succeed. When protosulphate of iron was added to the solution of the bleaching salt, it acquired a deep red colour from a few drops, and it speedily became impossible for me to determine whether I had added a sufficient quantity of protosulphate or not. Mr. Dalton has not been explicit enough in his description of his mode of analysis, to enable other chemists to repeat his process. Of course I am unable to state, whether the difference between Mr. Dalton's results and mine be owing to the different modes of analysis which we followed, or to a difference in the composition of the bleaching powders which we examined. I think it very likely that both causes contributed to produce the difference which exists between us.

II. *Nature of Oxymuriate of Lime.*

But the principal object of my experiments on the bleaching powder was to ascertain the nature of the compound formed when chlorine is made to pass through hydrate of lime.

1. 1160 gr. of the dry powder were put into a glass retort, the beak of which was luted to a bent tube, the end of which was plunged into a water trough, and a glass jar full of water was inverted above it. The retort being heated on a sand bath, gas came over, and continued to come over for some hours. The whole quantity thus extricated measured 164 cubic inches, and was pure oxygen gas. The dry salt in the retort had lost its smell and its action on vegetable colours; and when digested in water, a solution of common muriate of lime was obtained. I conclude from this experiment that, in the bleaching salt, the

chlorine was united not to calcium, but to lime; and that, therefore, the bleaching salt of Mr. Tennent is in reality a *chloride of lime*, as it has hitherto been supposed to be. When it is heated, the lime parts with its oxygen, and is converted into calcium, and the chloride of lime becomes a chloride of calcium. Of course it loses its peculiar properties, and, when dissolved in water, is nothing else than a muriate of lime.

Hence the reason that during the preparation of the bleaching powder, it is necessary to keep the temperature of the lime very low. If it be allowed to acquire heat, the chloride of lime is converted into chloride of calcium, and becomes useless for the purposes of the bleacher. Probably unslacked lime might be united with chlorine, if its temperature could be kept low. But when the attempt is made on a large scale, so much heat is always generated that the lime is speedily converted into calcium, and the object frustrated.

2. I find that barytes, strontian, potash, and soda, may be united to chlorine as well as lime, so that chlorides of these bases exist. They are easily obtained by double decomposition from chloride of lime. When heated, they give out oxygen gas, and are converted into chlorides of barium, strontium, potassium, and sodium. It is probable that many of the metallic oxides are capable of forming chlorides likewise. Indeed from the trials which I made, I have little doubt that solution of chloride of lime may be employed with advantage to procure several of the metallic chlorides in quantities and with facility. But the discussion of these and many other points, I must leave till a future opportunity.

ARTICLE III.

On the Reduction of Lunar Distances for finding the Longitude.

By Dr. Tiarks.

(To Dr. Thomson.)

SIR, *Chateangay Woods, North America, Sept.* 19, 1818.

THE method of determining the longitude by observations of lunar distances is by far not so commonly practised as it might be expected, considering the number of instruments fit for such purposes which are in common use. The calculations which such observations require are a great obstacle with most people. Mendoza's tables, by which they are very much abridged, do not seem to be in general use; and the methods contained in the common books, besides being often very inaccurate, require not unfrequently more labour and rules than the direct formulæ. Seamen commonly compute by different methods, in order to guard against mistakes; but in cases of a disagreement of the results, they are uncertain in which calculation the mistake lies, and, as I have observed myself, have often neither patience nor time enough to find out the errors which are easily committed in logarithmic calculations.

It appeared, therefore, to me, that a method susceptible of an easy *check*, like those which Prof. Gauss has introduced into astronomical calculations, would be desirable both for astronomers and navigators. The method which I propose for this purpose seems to deserve notice; and I can recommend it the more as the practical seamen to whom I have had an opportunity of communicating it, have found it easy and useful.

Reduction of the apparent Distances of the Moon from a celestial Body to the true Distance.

Let be

$$\text{☽'s altitude} \begin{cases} \text{apparent} = h \\ \text{true} \quad = h' \end{cases} \quad \text{☉'s or stars altitude} \begin{cases} \text{appar.} = \text{H} \\ \text{true} \quad = \text{H}' \end{cases}$$

$$\text{Distance} \begin{cases} \text{apparent} = \text{D} \\ \text{true} \quad = \text{D}' \end{cases} \quad \frac{\text{H} + h + \text{D}}{2} = \text{S}.$$

and the angle at the zenith in the triangle formed by the moon, the celestial body, and the zenith = Z.

We have immediately the two following equations:

$$\text{Sin.}\, h\, .\, \text{sin. H} + \text{cos.}\, h\, .\, \text{cos. H}\, .\, \text{cos. Z} = \text{cos. D}$$

$$\text{Sin.}\, h'\, .\, \text{sin. H}' + \text{cos.}\, h'\, .\, \text{cos. H}'\, .\, \text{cos. Z}' = \text{cos. D};$$ therefore,

$$\text{cos. Z} = \frac{\text{cos. D} - \text{sin.}\, h\, .\, \text{sin. H}}{\text{cos.}\, h\, .\, \text{cos. H}} = \frac{\text{cos. D}' - \text{sin.}\, h'\, .\, \text{sin. H}'}{\text{cos.}\, h'\, .\, \text{cos. H}'} \,\ldots\ldots (\alpha)$$

From this is easily derived,

$$\text{cos. D}' = \text{sin.}\, h'\, .\, \text{sin. H}' + \frac{\text{cos.}\, h'\, .\, \text{cos. H}'}{\text{cos. H}\, .\, \text{cos. H}}\, .\, \{\text{cos. D} - \text{sin.}\, h\, .\text{sin.H}\}$$

and from this by adding and subtracting on the left cos. H′ . cos. h'

$$\text{cos. D}' = \text{cos.}\, (\text{H}' - h') + \frac{\text{cos.}\, h'\, .\, \text{cos. H}'}{\text{cos.}\, h\, .\, \text{cos. H}} \{\text{cos. D} - \text{cos.}\, (\text{H} - h)\}$$

but cos. D − cos. (H − h) being equal $- 2\, \text{sin.} \left(\frac{\text{D} + \text{H} - h}{2}\right) .\, \text{sin} \left(\frac{\text{D} - \text{H} + h}{2}\right)$

we have likewise,

$$\text{cos. D}' = \text{cos}\, (\text{H}' - h') - 2\, \frac{\text{cos.}\, h'\, .\, \text{cos. H}'}{\text{cos.}\, h\, .\, \text{cos. H}}\, .\, \text{sin.} \left(\frac{\text{D} + \text{H} - h}{2}\right) .\, \text{sin.} \left(\frac{\text{D} - \text{H} + h}{2}\right) \text{ or}$$

$$= \text{cos.}\, (\text{H}' - h') - 2\, \frac{\text{cos.}\, h'\, .\, \text{cos. H}'}{\text{cos.}\, h\, .\, \text{cos. H}}\, .\, \text{sin.}\, (\text{S} - h)\, .\, \text{sin.}\, (\text{S} - \text{H}) \,\ldots\ldots\ldots\ldots (\beta)$$

Suppose

$$\frac{\text{cos.}\, h'\, .\, \text{cos. H}'}{\text{cos.}\, h\, .\, \text{cos. H}}\, \text{sin.}\, (\text{S} - h)\, .\, \text{sin.}\, (\text{S} - \text{H}) = \text{cos.}\, \varphi^2, \text{ or}$$

$\sqrt{\left[\frac{\cos. h' . \cos. H'}{\cos. h . \cos. H} \sin. (S - h) \sin. (S - H)\right]} = \cos. \varphi$ (A), and we have by (β)

$\cos. D' = \cos. (H' - h') - 2 \cos. \varphi^2$, or by substituting $1 + \cos. 2 \varphi$ for $2 \cos. \varphi^2$

$\cos. D' = \cos. (H' - h') - 1 - \cos. 2 \varphi$ and

$1 + \cos. D' = \cos. (H' - h') - \cos. 2 \varphi$, which gives

$$2 \cos. \tfrac{1}{2} D'^2 = 2 \sin \left(\varphi + \left(\frac{H' - h'}{2}\right)\right) . \sin. \left(\varphi - \left(\frac{H' - h'}{2}\right)\right)$$

consequently

$$\cos. \tfrac{1}{2} D' = \sqrt{\sin. \left(\varphi + \left(\frac{H' - h'}{2}\right)\right) . \sin. \left(\varphi - \left(\frac{H - h'}{2}\right)\right)} \;..\; \text{(B)}$$

We have by (α)

$$\frac{\cos. D - \sin. h . \sin. H}{\cos. h . \cos. H} = \frac{\cos. D' - \sin. h' . \sin. H'}{\cos. h' \cos. H'}$$

But

$$\frac{\cos. D - \sin. h . \sin H}{\cos. h . \cos. H} - 1 = \frac{\cos. D - \cos. h . \cos. H - \sin. h . \sin. H}{\cos. h . \cos. H} =$$

$$\frac{\cos. D - \cos. (H - h)}{\cos. h . \cos. H} = \frac{2 \sin. \left(\frac{D + H - h}{2}\right) . \sin. \left(\frac{D - H + h}{2}\right)}{\cos. h . \cos. H} \ldots$$ In the same manner will be $\frac{\cos. D' - \sin. h' . \sin. H'}{\cos. h' . \cos. H'} - 1 =$

$$\frac{2 \sin. \left(\frac{D' + H' - h'}{2}\right) . \sin. \left(\frac{D' - H' + h'}{2}\right)}{\cos. h' . \cos. H'}$$

Hence $\frac{\sin. \left(\frac{D + H - h}{2}\right) \sin. \left(\frac{D - H + h}{2}\right)}{\cos. h . \cos. H} =$

$\frac{\sin. \left(\frac{D' + H - h}{2}\right) \sin. \left(\frac{D - H + h}{2}\right)}{\cos. h' . \cos. H'}$, or

$$\frac{\sin (S - h) . \sin. (S - H) \cos. h' . \cos. H'}{\cos. h . \cos. H} = \sin. \left(\frac{D'}{2} + \left(\frac{H' - h'}{2}\right)\right) . \sin. \left(\frac{D'}{2} - \left(\frac{H' - h'}{2}\right)\right) \ldots\ldots \text{(C)}$$

It may be remarked, that when $h' > H'$, $\frac{h' - H'}{2}$ is to be substituted in the equations B and C.

The equation (A) gives φ, and D′ is found by (B). The term, to the left, (of the equation (C)) is the same as $\cos. \varphi^2$ previously found, which must be equal to the other term of (C), for the calculation of which, D′ is required. If, therefore, D′ as found by (B), makes this term equal to $\cos. \varphi^2$, the calculation is correct, otherwise not. An example may illustrate the whole.

$h = 17° 47' 27''$

$H = 56\ 16\ 50$ $\quad H' = 56° 16' 18''$

$D = 101\ 46\ 43$ $\quad h' = 18\ 38\ 43$

175 51 30

$S = 87\ 55\ 30$

$h = 17\ 47\ 27$ ..comp. cos. = 0·0212817

$H = 56\ 16\ 50$ ..comp. cos. = 0·2556070

$S - h = 70\ 08\ 03$ ..sin. = 9·9733546 $\quad \frac{1}{2}D' = 50° 29' 00·0''$

$S - H = 31\ 38\ 40$ ..sin. = 9·7198667 $\quad \frac{H' - h'}{2} = 18\ 48\ 47·5$

$H' = 56\ 16\ 18$ ..cos. = 9·7444931 sum = 69 17 47·5 ..sin. = 9·9710079

$h' = 18\ 38\ 43$ ..cos. = 9·9765866 diff. = 31 40 12·5 ..sin. = 9·7201826

$H' - h' = 37\ 37\ 35$ ..cos. ϕ^2 = 9·6911906 $\quad$ 9·6911905

$\frac{H' - h'}{2} = 18\ 48\ 47·5$

ϕ 45 30 31·05 ..cos. ϕ = 9·8455953

Sum. = 64 19 18·55 ..sin. = 9·9548414

Diff. = 26 41 43·55 ..sin. = 9·6524859

cos. $\frac{1}{2}D'^2$ = 9·6073273

cos. $\frac{1}{2}D'$ = 9·8036637

$\frac{1}{2}D' = 50° 29' 00''$

$D' = 100\ 58\ 00$

It is clear that it would be very easy to prepare printed forms to be filled up, and that the calculations would become more accurate, and not liable to mistakes.

I am, Sir, your obedient servant,

T. L. Tiarks.

Article IV.

Mathematical Problems. By James Adams, Esq.

Stonehouse, Sept. 20, 1818.

Your inserting the following problems, &c. in the *Annals of Philosophy,* will much oblige your most obedient servant,

James Adams.

Problem 1.—To find the difference of the natural cosines of two arcs by logarithms.

Per trigonometry, $\cos. B - \cos. A = 2 . \sin. \frac{A + B}{2} . \sin. \frac{A - B}{2}$.

Problem 2.—To find (A + B) − B, by logarithms.

$(A + B) - B = \left(1 - \frac{B}{A + B}\right)(A + B)$. Find an arc corresponding to the log. sin. $\sqrt{\frac{B}{A + B}}$, which denote by C;

Then 2 log. cos. C + log. (A + B) = log. { (A + B) − B }.

This solution depends on the property $\sin.^2 A + \cos.^2 A = \text{rad.}^2$

Problem 3.—To find 1 − (1 + *m*) by logarithms.

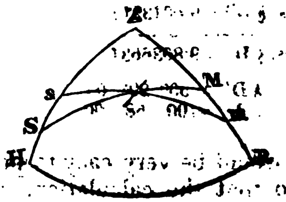
$1 - (1 + m) = -\{(1 + m) - 1\} = -\left(1 - \frac{1}{1 + m}\right)(1 + m)$.

Find an arc corresponding to the log. sin. $\sqrt{\frac{1}{1 + m}}$, which represent by D;

Then − { 2 log. cos. D + log. (1 + *m*) } = log. { 1 − (1 + *m*) }.

This solution also depends on the property $\sin.^2 A + \cos.^2 A = \text{rad.}^2$.

Problem 4.—To reduce the observed distance of the moon and sun, or moon and star, to the true, by the *addition* of log. sines, and cosines, only.



Let M and S represent the true places of the moon and sun, or star; *m* and *s* the apparent places, *m s* the apparent distance, M S the true distance, Z the zenith, and H R the horizon of the place of observation.

Put half the sum of the apparent distance and difference of apparent altitudes........................ = A

Half their difference .... = B

And the difference of the true altitudes .... = C

Then per Simpson's Trigonometry, p. 74.

$$\frac{\cos.(Z m - Z s) - \cos. s m}{\sin. Z m . \sin. Z s} = \frac{\cos.(Z S - Z M) - \cos. S M}{\sin. Z S . \sin. Z M}$$

From whence

$$\cos. S M = \cos.(Z S - Z M) - \left\{\frac{\cos.(Z m - Z s) - \cos. s m}{\sin. Z m . \sin. Z s}\right\}$$
$$\sin. Z S . \sin. Z M$$

Or,

$$\cos. S M = \cos.(M R - S H) - \left\{\frac{\cos.(s H - m R) - \cos. s m}{\cos. m R . \cos. s H}\right\}$$
$$\cos. S H . \cos. M R$$

Or by Problem 1,

$$\cos. S M = \cos. C - \frac{2 \sin. A . \sin. B . \cos. S H . \cos. M R}{\cos. m R . \cos. s H}$$

Or,

$$\cos. S M = \left(1 - \frac{2 \sin. A . \sin. B . \cos. S H . \cos. M R}{\cos. m R . \cos. s H . \cos. C}\right) \cos. C$$

Then by Problem 2 (when the apparent distance is less than 90°), we have

Rule 1.—Log. 2. .... =
sin. A. .... =
sin. B. .... =
cos. star's true alt. .... =
cos. moon's true alt. .... =
cos. star's app. alt. (ar. com.) . =
cos. moon's app. alt. (ar. com.) . =
cos. C (ar. com.) .... =

Sum of logarithms .... =
Half sum, corresponding to sin. D =

2 log. cos. D. .... =
log. cos. C. .... =

cos. true distance. .... =

And by Problem 3 (when the apparent distance is greater than 90°), we have

Rule 2.—Log. 2. .... =
sin. A. .... =
sin. B. .... =
cos. star's true alt. .... =
cos. moon's true alt. .... =
cos. star's app. alt. (ar. com.) .. =
cos moon's app. alt. (ar. com.) . =
cos. C (ar. com.) .... =

Sum of logarithms .... =
Half ar. com. of sum corresponding to sin. D .... } =

2 log. cos. D. .... =
cos. C .... =
sum of logs. as above .... =

cos. sup. of true distance. ..... =

Note 1.—The difference between the apparent and true distance can never exceed one degree. (Emerson's Miscellanies, page 206.)

Note 2.—The error of one second in calculating the moon's distance will produce an error of half a mile in longitude.

The following examples will show how very simple the opera-
ns by these rules are.

Example 1.—Given the apparent altitudes of the moon and
n 27° 2′ 30″ and 59° 11′ 52″; the true altitudes 27° 53′ 25″,
° 11′ 22″, and the apparent distance 59° 25′ 34″; to find the
ıe distance.

From the data

A = 45° 47′ 28″, B = 13° 38′ 6″, and C = 31° 17′ 57″.

Then per Rule 1.

ıg. 2.			=	0·3010300
sin. A	=45° 47′ 28″		=	9·8553995
sin. B	=13 38 6		=	9·3724256
cos. sun's true alt.	=59 11 22		=	9·7094405
cos. moon's true alt.	=27 53 25		=	9·9463761
cos. star's app. alt.	=59 11 52	(ar. com.)	=	0·2904536
cos. moon's app. alt.	=27 2 30	(ar. com.)	=	0·0502802
cos. C	=31 17 57	(ar. com.)	=	0·0683050
Sum of logarithms			=	19·5937105
Half sum corresponding to sin. D	=*38 47 8		=	9·7968552
2 log. cos. D			=	9·7836276
cos. C.			=	9·9316950
cos. true distance	=54 43 20		=	9·7153226

The same as determined by Mr. Sanderson's rule in the
ıdies' Diary for 1787, but 9″ less than is given at page 47,
equisite Tables, whence the example is taken.

Example 2.—Given the apparent altitudes of the moon and
ar 28° 29′ 44″ and 45° 9′ 12″; the true altitudes 29° 17′ 45″,
° 8′ 15″; and the apparent distance 63° 35′ 13″; to find the
ue distance.

From the data

A = 40° 7′ $20\frac{1}{2}$″, B = 23° 27′ $52\frac{1}{2}$″, and C = 15° 50′ 30″.

Then per Rule 1.

ɔg. 2.			=	0·3010300
sin. A	=40° 7′ $20\frac{1}{2}$″		=	9·8091704
sin. B	=23 27 $52\frac{1}{2}$		=	9·6000818
cos. star's true alt.	=45 8 15		=	9·8484403
cos. moon's true alt.	=29 17 45		=	9·9405687
cos. star's app. alt.	=45 9 12	(ar. com.)	=	0·1516804
cos. moon's app. alt.	=28 29 44	(ar. com.)	=	0·0560832
cos. C.	=15 50 30	(ar. com.)	=	0·0168160
Sum of logarithms			=	19·7238708

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* There is no necessity for taking out the arc D; for having found the half sum
ıongst the log. sines, the log. cosines even therewith may be easily seen, and its
uble taken out at once.

Half sum corresponding to sin. D	=46° 41′ 29″	...... =	9·8619354
2 log. cos. D.		.... =	9·6725568
cos. C.		.... =	9·9831840
cos. true distance..	=63 5 15	...... =	9·6557408

The same as determined by Mr. Sanderson's rule. By Borda's theorem, it is 63° 5′ 8″. See Dr. Gregory's Trigonometry, page 179.

Example 3.—Given the apparent altitudes of the moon and sun 22° 15′, and 21° 35′; the true altitudes 23° 6′ 22″, 21° 32′ 44″, and the apparent distance 119° 20′ 34″; to find the true distance.

From the data

A = 60° 0′ 17″, B = 59° 20′ 17″, and C = 1° 33′ 38″.

Then per Rule 2.

Log. 2				=	0·3010300
sin. A	=60°	0′	17″	=	9·9375513
sin. B	=59	20	17	=	9·9345950
cos. sun's true alt.	=21	32	44	=	9·9685417
cos. moon's true alt.	=23	6	22	=	9·9636838
cos. sun's app. alt.	=21	35	0 (ar. com.)	=	0·0315714
cos. moon's app. alt.	=22	15	0 (ar. com.)	=	0·0336046
cos. C	= 1	33	38 (ar. com.)	=	0·0001611
Sum of logarithms				=	0·1707389
Half ar. com. of sum corresponding to sin. D	=55	14	23	=	9·9146305
2 log. cos. D				=	9·5119694
cos. C				=	9·9998389
sum of logs. as above				=	0·1707389
cos. sup. true dist.	= 61	13	12	=	9·6825472
true distance	=118	46	48		

The same as given in Dr. Kelley's Spherics, p. 184, and as determined by Mr. Sanderson's rule.

If the logarithmic sines and cosines be taken from Mr. Michael Taylor's very valuable tables, the solution of the problem would then be rendered very easy; but I am apprehensive that the necessary high price of those excellent tables prevents their being used as often as they otherwise would be. This inconvenience might, in a great degree, be obviated, by printing the sines and cosines only in one volume of a convenient size; which is all that is here required. Indeed if Government were to keep such tables in store, and supply on moderate terms those officers with them whose duty it may be to determine the longitude of a ship at sea, it would, in my opinion, render the working of a *lunar observation* considerably more correct, more general, and less laborious than at present; for a person with a very moderate knowledge of arithmetic might learn the use of such tables and the above-mentioned rules in a few hours.

Errata in No. LXIX, *Sept.* 1818.

Page 205, line 16, in the numerator, *read* $x^{m-1}(a+bx^n)^p-(m-n)\int ac$

Page 206, line 2, *read* $+\frac{a^2x^{m-2}}{m-2}+\frac{a^3\int x^{m-3}dx}{x-a}$

Page 207, line 26, in the numerator, *read* $(a+cz^n)^{m+1}\times d$

Page 208, line 2, in the last term of the numerator, *read* $(a+cz^n)^m$

Page 208, line 7, in the last term of the numerator, *read* $(a+cz^n)^m$

Page 208, line 18, *read* $=$ I.

In pages 204 and 205, change *Formula* into *Formulæ* where necessary.

Article V.

On the Maxima and Minima of Quantities (*by common Algebra*).

By Mr. Thomas Slee.

Terril, near Penrith, Jan. 13, 1818.

The design of the following paper is to enlarge the province of common algebra, by extending it to the maxima and minima of analytical functions. Should it appear to you to be new, and of sufficient importance to merit a place in the *Annals of Philosophy*, the insertion of it in a future number will much oblige

Thomas Slee.

Lemma.—Let y be any function of a variable x represented by fx, and admitting of a maximum or a minimum; then in the equation $fx=y$, x has always two *different* affirmative values, except when y is a max. or a min. and in that case these values are equal to each other; or if m denote the greatest or least value of y, the equation $fx=m$ has two equal affirmative roots.

The truth of the lemma may be shown in the following manner. Every function of a variable may be represented by the ordinate of a curve. Let then M D M′ be the curve of which the equation is $fx=y$, the abscissa A P being denoted by x, and the ordinate P M by y, and in fig. 1 let D C be the greatest ordinate possible. Then P M first increases till it becomes equal to C D, and then it decreases. Consequently a line drawn from M parallel to the axis A B, will cut the curve again in some point M′ on the other side of C D. Draw M′ P′ parallel to M P; then M′ P′ = M P. Therefore, corresponding to any particular value P M of the ordinate, there are two *different* abscissa, viz. A P and A P′, which are both affirmative, being on the same side of the point A; that is, x has two different affirmative values in the equation $f x = y$. But conceive P M, and consequently P′ M′, to move up to C D, then A P and A P′ are each equal to A C, and, therefore, equal to each other, which shows that the two affirmative roots of the preceding equation become *equal* to each other when $y = m$. The same reasoning is evidently applicable to fig. 2, where the ordinate P M is supposed to admit of a minimum C D.

Fig. 1.

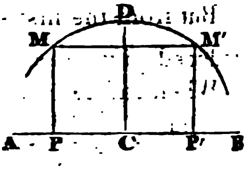
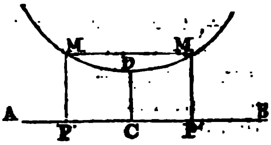


Fig. 2.



Corollary.—Hence if we deduce a value of x from the equation $f x = m$, upon the supposition of its having two equal roots, that value will correspond to a maximum or a minimum.

We shall now proceed to illustrate this theory, by applying it to a few examples; but we must first remind the reader of the rule, which is investigated in most works on algebra, for reducing equations having two equal roots. It is as follows: Multiply each term of the equation by the index of the unknown quantity in that term, diminish the index by unity, and equate the result to nothing. Thus if the equation $a x^n \mp b x^{n-1} \mp c x^{n-2}$ &c. $\mp m = o$, have two equal roots, it may be demonstrated that

$$n a x^{n-1} \mp \overline{n-1} . b x^{n-2} \mp \overline{n-2} . c x^{n-3} = o.$$

Problem 1.—To divide a given line or number (a) into two such parts that the rectangle under them may be a maximum (m).

Let x = one of the parts, then $a - x$ = the other, and $\overline{a - x} \times x = a x - x^2 = m$, or $x^2 - a x + m = o$.

This equation has two equal roots by the lemma,

Therefore, by the preceding rule, we have

$$2 x - a = o \therefore x = \frac{a}{2}.$$

Problem 2.—Find that fraction which, being diminished by its cube, shall give the greatest remainder possible. Let x be the required fraction, then $x - x^3 = m$ or $x^3 - x + m = o$. ∴ by the rule $3 x^2 - 1 = o$ and $x = \sqrt{\frac{1}{3}}$.

Problem 3.—Find x when $\frac{x^2}{x - a} = m$

Multiplying by $\overline{x - a}$, and transposing, the equation becomes $x^2 - m x + a m = o \therefore 2 x - m = o$ and $m = 2 x$.

But from the first equation $m = \frac{x^2}{x - a} \therefore \frac{x^2}{x - a} = 2 x$, which reduced gives $x = 2 a$.

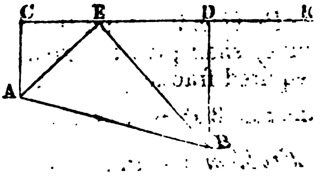
When radical quantities enter the proposed function, it does not appear that the common rule for reducing equations having two equal roots is generally applicable. We shall, therefore, investigate one by which examples of this kind may be resolved. Since the equation $f x = y$ has two affirmative roots, let $p =$ less = root and $\overline{p + e}$ = greater (e being their difference, and represented in the illustration of the lemma by the line P P′). Then p and $p + e$ substituted for x in the equation $f x = y$ give the same result, viz. y. Therefore $f p = y = f\,(p + e)$ (A). After developing the second member of this equation, and taking away the quantities that are common to each side, all the remaining terms will be divisible by e, and we shall have an equation containing p, e, and constant quantities, which will be true for every value of the function. But when $y = m$, $e = o$; therefore all the terms containing e and its powers will vanish, and we shall have an equation expressing the relation between p and given quantities, from the resolution of which, p or its equal x will be known.

Let us apply this method to problem 2, where we have given $x - x^3 = y$, to find x when $y = m$. This equation has two affirmative roots (by the lemma). Let p = less root and $p + e$ = greater. Then these substituted for x in the proposed equation give the same result, viz. y. Therefore, $p - p^3 = y = p + e - (p + e)^3$, or $p - p^3 = p + e - p^3 - 3 p^2 e - 3 p e^2 - e^3$. Taking away $p - p^3$ from each side, and dividing the remaining terms by e, we have $1 - 3 p^2 - 3 p e - e^2 = o$. Now make $e = o$ (because when $y = m$, e vanishes), and the last equation becomes $1 - 3 p^2 = o \therefore p = \sqrt{\frac{1}{3}} = x$.

From this example, it is evident that in developing the functions of $(p + e)$ only two terms of the series need be taken, because the succeding ones contain e^2, e^3, &c. and, therefore, ultimately disappear.

Problem 4.—Given in position two points A, B, and the line C K; it is required to determine the point E in this line, so that (A E + B E) may be the least possible.

Fig. 3.



From the points A and B, let fall the perpendiculars A C, B D, upon C K, and let E be the required point. Put A C = a, B D = b, C D = c and C E = x; then E D = $c - x$, A E = $\sqrt{a^2 + x^2}$, B E = $\sqrt{(c - x)^2 + b^2}$.

Therefore $\sqrt{a^2 + x^2} + \sqrt{(c - x^2) + b^2} = y$, and it is required to find x, when $y = m$ (minimum).

Substituting p and $(p + e)$ for x, as in the last example, we have $\sqrt{a^2 + p^2} + \sqrt{(c - p)^2 + b^2} = y = \sqrt{a^2 + (p + e)^2} + \sqrt{(c - (p + e)^2 + b^2}$. Now $\sqrt{a^2 + (p + {}^2e)} = \sqrt{a^2 + p^2} + \frac{p\,e}{\sqrt{a^2 + p^2}}$ + &c. and $\sqrt{(c - (p + e))^2 + b^2} = \sqrt{(c - p^2) + b^2} - \frac{(c - p)\,e}{\sqrt{(c - p)^2 + b^2}}$ + &c.

Therefore $\sqrt{a^2 + p^2} + \sqrt{(c - p)^2 + b^2} = \sqrt{a^2 + p^2} + \frac{p\,e}{\sqrt{a^2 + p^2}}$ + &c. + $\sqrt{(c - p)^2 + b^2} - \frac{(c - p)\,e}{\sqrt{(c - p)^2 + b^2}}$ + &c.

Taking away the quantities that are common to each side and dividing by e, we have $\frac{p}{\sqrt{a^2 - p^2}} - \frac{c - p}{\sqrt{(c - p)^2 + b^2}} = o$, which reduced gives $p = \frac{a\,c}{a + b} = x$.

Emerson, in his Algebra, has given a rule very analogous to this, for resolving problems relating to the maxima and minima; but it appears to have been suggested to him by the differential calculus. At any rate his notion of the symbol e is very different from ours; for he supposes it to represent an infinitely small quantity, and he rejects the terms containing its powers, without assigning a very satisfactory reason. We conceive e to be the difference of two affirmative roots of the equation $f\,x = y$, which is always a real finite quantity, except when y arrives at a limit, and then it actually vanishes.

We shall conclude this article with a concise demonstration of the well-known theorem, that the fluxion or differential of a function $= o$, when it is a maximum or minimum. For developing the second term of the equation marked (A), by Taylor's theorem,* we have

$$f\,p = f\,(p + e) = f\,p + \frac{d\,f\,p}{d\,p}\,e + \frac{d^2\,f\,p}{d\,p^2}\,e^2 + \&c.$$

Taking away $f\,p$ from each side of the equation, and dividing by e, we have $\frac{d\,f\,p}{d\,p} + \frac{d^2\,f\,p}{d\,p^2}\,e + d = o$. But when the function attains a limit $e = o$, and, therefore, all the terms, except the first, disappear from this equation; and we have in this case $\frac{d\,f\,p}{d\,p} = o$. But $p = x$ $\therefore \frac{d\,f\,x}{d\,x} = o$, or $d\,f\,x = o$.

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[*] For a demonstration of Taylor's theorem, see Calcul. differential et integral de Lacroix.

ARTICLE VI.

Examination of the Influence of the Time of the Day upon Barometrical Measurements. Extracted from the Researches of M. Delcros.*

M. DELCROS undertook to resolve this problem: "Suppose two barometers, separated from each other by a certain space, both horizontal and vertical; at what time of the day ought they to be observed, that the height of the stations resulting from calculation may approach the nearest to accuracy?"

To obtain the solution, M. Delcros made choice of two stations, conveniently situated for observing at the same time two barometers, well constructed, at five different periods of the day, each separated from the other by an interval of two hours; namely, at eight o'clock in the morning, ten o'clock, noon, two o'clock, and four o'clock in the evening. One of the stations was at Strasburg, in the cabinet of M. Herrenschneider, Professor of Natural Philosophy in the Academy, and a very accurate observer. The other station was the castle of Lichtemberg, upon an insulated summit of the Vosges, about ten leagues north from Strasburg, and about 264·35 metres above that city, and connected with it by one of the triangles belonging to the great base of Ensisheim. Colonel Henry, who had the superintendence of the geodesical observations executing in the east of France, had resolved to make a set of astronomical observations at Lichtemberg, to determine the amplitude of the celestial arc of the meridian from Geneva to Luisberg, an arc which this point divides into two parts nearly equal. M. Delcros being obliged to make a considerable abode at Lichtemberg, in order to assist his superior, took advantage of the opportunity to make a complete series of barometrical observations in the same place at the five times of the day above-mentioned. These observations were simultaneous with others made at Strasburg by Professor Herrenschneider. They were continued for 22 days, which gives 100 observations to compare, disposing them in five groupes of corresponding observations, which, "may be compared with each other. This comparison has been carefully made by M. Delcros, which, in his opinion, may add some rays to the luminous pencil collected by the celebrated philosopher Ramond, to whom the barometrical method is indebted for so many labours—for so many profound investigations—for so many precious memoirs, in which he has united the principles of a simple and luminous philosophy with the charms of style."——

These observations have been arranged by the author in two very interesting tables.

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* Translated from the Bibliotheque Universelle, vii. 236. (April, 1818.)

In the first, divided into 14 columns, and which we are prevented from publishing by its great size, we find the dates (days and hours), the heights of the barometer observed, and the temperature of the mercury and the air at Lichtemberg; the same elements for Strasburg; the numbers given by the tables of Oltmans; the corrections for the temperature of the mercury and the air; for the latitude, for the diminution of gravity in the vertical; the difference between the heights of the barometers, derived from the calculation of each of the corresponding observations; and, finally, all the meteorological circumstances that accompanied each observation. This table contains all the elements of the second, the object of which is to show the influence of the time of the day, by the way in which the results are grouped. This table accompanies the present article. Its general title, and that of its several columns, indicate sufficiently its object. We perceive the results of each observation grouped respectively into each of the horary epochs that furnished it: at the end of each observation is given, in metres and centimetres, the quantity by which it differs from the true height of the station as determined geometrically. At the bottom of each of the five columns of these differences is given the mean number of metres round which the results oscillate; and the *greater* this number is, the *less* is the time of the day which it represents favourable for accuracy. But by casting the eye over the bottom of the five columns, we shall perceive the results of which we form this very instructive little table.

Hours of simultaneous observation.	Mean errors of the results. Metres.	
8 a.m	−3·58	Result of 110 observations compared.
10	−2·23	
Noon	−0·62	
2 p.m	−0·59	
4	−1·02	

That is to say, that if we choose eight in the morning for the simultaneous observations of two barometers placed as above stated, we have the mean chance of an error of 3½ metres in the 264; that is to say $\frac{1}{76}$th of the whole; at noon the error is only 0·62, or $\frac{1}{426}$, which is very small; but at two o'clock, p. m. the error is still less, being only 0·59 in the 264 metres, or $\frac{1}{452}$.

trically to be 264·35 metres.

Diurnal Epochs.	Horary Epochs.										Mean diurnal differences in height.	Mean diurnal errors.
	8 in the morning.		10 a.m.		Noon.		2 p.m.		4 p.m.			
	Difference of height.	Horary errors.	Difference of height.	Horary errors.	Difference of height.	Horary errors.	Difference of height.	Horary errors.	Difference of height	Horary errors.		
1812. Nov. 4	260·52	− 3·83	268·40	+ 4·05	268·14	+ 3·79	272·20	+ 7·85	270·56	+ 6·21	267·96	+ 3·61
5	277·95	+13·60	274·00	+ 9·65	275·24	+10·89	273·28	+ 8·93	270·01	+ 5·66	274·10	+ 9·75
6	264·05	− 0·30	262·39	− 1·96	267·18	+ 2·83	269·26	+ 4·91	266·04	+ 1·69	265·78	+ 1·43
7	260·62	− 3·73	260·84	− 3·51	260·87	− 3·48	265·61	+ 1·26	265·70	+ 1·35	262·73	− 1·62
8	266·73	+ 2·38	266·47	+ 2·12	270·11	+ 5·76	268·14	+ 3·79	267·69	+ 3·34	267·83	+ 3·48
9	259·15	− 5·20	263·61	− 0·74	263·76	− 0·59	265·40	+ 1·05	262·42	− 1·93	262·87	− 1·48
10	257·05	− 7·30	253·41	−10·94	258·93	− 5·42	262·35	− 2·00	263·28	− 1·07	259·00	− 5·35
11	259·02	− 5·33	259·92	− 4·43	263·89	− 0·46	262·75	− 1·56	264·72	+ 0·37	262·06	− 2·29
12	259·67	− 4·68	264·18	− 0·17	265·10	+ 0·75	257·40	− 6·95	262·08	− 2·27	261·69	− 2·66
14	260·48	− 3·87	262·50	− 1·85	264·74	+ 0·39	263·32	− 1·03	265·69	+ 1·34	263·35	− 1·00
15	267·79	+ 3·44	268·37	+ 4·02	268·78	+ 4·43	265·86	+ 1·51	265·10	+ 0·75	267·18	+ 2·83
16	261·13	− 3·22	260·51	− 3·84	260·56	− 3·79	260·15	− 4·20	262·48	− 1·87	260·97	− 3·38
17	265·13	+ 0·78	265·88	+ 1·53	269·68	+ 5·33	261·95	− 2·40	260·13	− 4·22	264·55	+ 0·20
18	262·30	− 2·05	265·46	+ 1·11	263·74	− 0·61	262·38	− 1·97	261·23	− 3·12	263·02	− 1·33
19	257·95	− 6·40	259·34	− 5·01	258·91	− 5·44	258·97	− 5·38	264·71	+ 0·36	259·98	− 4·37
20	255·61	− 8·74	256·61	− 7·74	259·64	− 4·71	262·49	− 1·86	260·28	− 4·07	258·93	− 5·42
21	255·01	− 9·34	251·47	−12·88	254·33	−10·02	256·78	− 7·57	257·04	− 7·31	254·93	− 9·42
22	251·21	−13·14	261·54	− 2·81	259·21	− 5·14	256·99	− 7·36	258·37	− 5·98	257·46	− 6·89
23	256·94	− 7·41	253·94	−10·41	261·54	− 2·81	257·82	− 6·53	259·36	− 4·99	257·92	− 6·43
24	260·08	− 4·27	265·85	+ 1·50	264·34	− 0·01	260·06	− 4·29	263·20	− 1·15	262·71	− 1·64
25	260·45	− 3·90	262·86	− 1·49	265·42	+ 1·07	265·52	+ 1·17	262·87	− 1·48	263·42	− 0·93
26	258·09	− 6·26	259·12	− 5·23	258·08	− 6·27	273·97	+ 9·62	260·30	− 4·05	261·91	− 2·44
Sums	1336·93		1306·67		1402·19		1402·65		1393·26		1380·85	
Horary means	260·77	− 3·58	262·12	− 2·23	263·74	− 0·61	263·76	− 0·59	263·33	− 1·02		

General mean of the 110 results. .... 262·74

Mean general error of the 110 results. .... − 1·61

ARTICLE VII.

An Account of some Experiments to ascertain the Strength of such Cast-Iron Shafts as are commonly used in Mill-Work, and what Proportion their Strength has to their Diameters. By Mr. Dunlop, of Glasgow.

WHOEVER remembers the kind of machinery which we had in this country about 25 years ago, will easily perceive, that, independently of other contrivances, it has been greatly improved by merely using cast-iron as a substitute for wood in the construction of it. All our other improvements have been limited; confined to particular machines; but this, having increased the strength and durability of every machine, has improved the whole.

That an improvement now so obvious and so important was not sooner observed and generally adopted is unaccountable; and our practitioners in the mechanical arts, in place of taking merit to themselves for the discovery, seem rather to owe an apology to their country for the slowness of their proceedings. The difference of expense was at one time a serious consideration, but did not continue long; for the price of timber advanced, whilst that of cast-iron remained about the same. The principal reason, perhaps, for our not using that metal in preference to wood was, its being easily broken, especially if in small pieces, by a jerk or a smart blow: wood, on the contrary, bends; and if not greatly overstrained, continues to bend for a long time before it entirely gives way; and thus indicating its want of strength by its flexibility, gives time to have any part of a machine constructed of wood either repaired or replaced. Cast-iron, on the contrary, gives no such indication of want of strength; if overstrained, it snaps in an instant, and endangers, perhaps, the lives of the people employed to work the machinery. It was on this account that our mechanics were cautious in using it as a substitute for wood; and when, in the course of practice, they had to judge of its strength, they were, as in similar cases, generally led by their own experience and observation; but to get experience, required time; and hence the slow progress which was made in this improvement. Even Bolton and Watt seem to have had little confidence in the strength of cast-iron, particularly if subjected to a jerk, and continued for many years to make the beams of their steam engines of wood. They employed wooden frames too for supporting their cylinders and crank-shafts; even their connex[illegible] rods and condenser cisterns were made of wood. It is no way astonishing then that our ordinary mechanics, many of whom had neither the science nor the practical skill of Bolton and Watt, were cautious. They had good reason, seeing these gentlemen, very justly regarded as at the head of their profession, using as little cast-iron in the construction of their engines as possible. But it may be said, "why so much caution and hesitation about a matter of this kind? Why was not the question put beyond all doubt by actual experiments. We have been in the habit of ascertaining the lateral strength of cast-iron by means of apparatus constructed for the purpose; for instance, all joists used in fire-proof buildings are proved by lateral pressure, why not then the beams of steam-engines in the same manner?"

The torsional strength too of such shafts as are used in millwork can be proved by wrenching. These reflections are natural enough; and I was aware that experiments had been made to ascertain the facts proposed, although I knew little of the results; and even if I had, still the necessity of my making similar experiments would not have been superseded, for this reason, that having occasion for a number of shafts in the summer of last year, my object was not to ascertain the strength of cast-iron shafts generally, but the strength of these particular shafts as nearly as possible, and to have them proved in such a manner as to be certain that they were of the strength required.

I had, therefore, an apparatus fitted up for the purpose, which merely consisted of two pine-logs, with a strong square socket of cast-iron fixed to the side of each, about two feet from the end; one of them served to hold the shaft, whilst the other, having an iron hook fixed to the end of it, 14 feet 2 inches from the centre of its socket, acted as a lever to wrench the shafts. These logs, supported about four feet from the ground, lay level and parallel with each other; and whilst the one end of the shaft was held by the one socket, the other end of it rested with its collar upon the edge of a plank, and the lever was applied upon the square of the shaft, and close to the side of the plank, so as to prevent as much as possible any lateral stress. The weight of the lever, or rather its effective weight, was ascertained by letting its hook rest on the scale of a balance, whilst its other end was supported upon a knife edge in the middle of the socket, and its effective weight thus ascertained was 120 lbs. To the hook at the end of the lever, in making the experiments, weights were suspended, and increased by not more than 2 lbs. at a time. Having got this apparatus finished, in order to ascertain the strength of such shafts as are usually cast in Glasgow, two bars of cast-iron, about five feet long each, the one 3, and the other 4¼ inches square, were turned in a lathe at five different places, and each place differed in diameter from the next to it by a quarter of an inch. Upon the cylindrical parts of these shafts, the experiments were made, and the following are the particulars.

No. of experiment.	Diameter of square part of shaft in inches.	Diameter of round part of shaft in inches.	Length of lever in feet and inches.	Weight of lever in lbs.	Weight on end of lever when the shaft broke.	Total weight which broke the shaft.	Length of round part of shaft in inches.	Length of fracture in inches.	Angle of fracture, or torsion, only seven of them being measured.	Cubes of diameter of shafts, round part.	Weight which should have broken the shaft, if its strength had been in proportion to the cube of its diameter, taking the weight which broke the two inch shaft as the standard of comparison.
			Ft. In.								
1	3	2	14 2 or 170 inches.	120	130	250	2¾	2¾	33°	8,000	250,000
2	3	2¼			264	381	3¼	3¼	37	11,390	356,000
3	3	2½			288	408	3	3		15,625	488,280 *
4	3	2¾			580	700	3	4½	39	20,800	650,000
5	3	3			†						
6	4½	3¼			1050	1170	4	4	36	34,328	1072,625
7	4½	3½			1120	1240	5	7	37	42,875	1339,840 *
8	4½	3¾			1542	1662	5	5	37		1647,937
9	4½	4			1818	1938	5	8			2000,000
10	4½	4¼			2038	2158	6	6	39		2398,750

The above are the particulars of nine experiments made in Cloud, Girdwood, and Co.'s Works, in 1818, with the view to ascertain the strength of cast-iron bars when subjected to twist or torsion; and it was from the result of these experiments, taken as data, that the diameters of the shafts for Broomwood Mill were resolved upon.

As the shafts on which the experiments were made were old ones and greasy, their being unsound was more easily perceived than if they had been new shafts.

Having ascertained these facts, the practical use of them is obvious and easy; for example, if it be required to find the diameter of such a shaft as will require the utmost force of a steam-engine of any given power to break it, let m = the momentum of the piston; that is, the pressure of the steam upon it in lbs. weight, multiplied into its velocity in feet per 1′, and let v = the velocity of the circumference of an imaginary pulley supposed to be fixed upon the first shaft employed to communicate motion to the greatest quantity of machinery which the engine is calculated to drive, and suppose the radius of this imaginary pulley equal to the length of the lever employed in these experiments, and a rope coiled round this pulley, we

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* Unsound, but in a small degree.

† In this experiment, the shaft having a large hole in it, broke with very little strain.

have only to find what weight suspended to the end of this rope multiplied into its velocity in feet per 1′, would equal the momentum of the piston; if x = this weight, then $v\,x = m$

$$x = \frac{m}{v}$$

which is, perhaps, as easy a method as any to find such a weight as would exactly balance the power of the engine. And referring to the abstract of the experiments, in the column entitled "Total Weight which broke the Shaft," we find the number of lbs. $= x$; and opposite to it, in the column entitled "Diameter of round Part of Shaft:" we have the diameter of the shaft required in inches; that is, however, the diameter of a shaft which would break with a force not less than the whole power of the engine.

It will then remain for the mill-wright, or his employer, to decide, upon how much stronger he would choose to have the shaft in question than the one already found in the abstract; and if he fixes upon having six times that strength, it will still be small, compared with most of those in Glasgow and the neighbourhood; for it would appear that most of them are unnecessarily strong, or at least unnecessarily heavy. But it may be said, why not have these shafts so strong as to put all risk of their breaking out of the question? This is certainly right; but having them unnecessarily strong, is attended not only with additional expense at first, but with a constant waste of power to drive them; besides, it does not follow that by increasing their diameters we increase their strength; for although large in diameter, and apparently sound, they may have very little strength, on account of air lodging in the heart of them at the time they were cast; hence the propriety of having all shafts proved by a wrenching apparatus, whatever may be their diameters; and in contracting for shafts, it would surely be better to have them at so much per lineal foot, of a certain strength, and not exceeding a certain weight, than by the cwt. which is the common practice.

Dec. 17, 1818.

Article VIII.

On Captain Cook's Account of the Tides in the Endeavour River.

In the 60th volume of the Philosophical Transactions there is a paper of Capt. Cook's on the tides, which he observed in part of the coast of New Holland.

About 11 o'clock in the evening of June 10, 1770, the Endeavour struck on a reef of coral rocks about six leagues from the land, on the east coast of that country. This happened about the time of high water, and the crew immediately began to lighten the vessel, in hopes of her floating at the next high tide; but it did not rise sufficiently by two feet to accomplish their wishes. They had now no hopes but from the tide at midnight, and these were only founded on a notion, very general, Captain Cook says, among seamen, that the tide rises in those seas higher by night than by day. The result exceeded their most sanguine expectations. The ship floated about 20 minutes after 10 in the evening, which was a full hour before high water. At this time, the heads of the rocks, which, during the preceding tide were at least a foot above water, were wholly covered. This circumstance led Captain Cook to attend to the tides, during the time (from June 17 to Aug. 4) which he lay in the Endeavour river, repairing the damages which the vessel had suffered; and he found that the neap tides were very inconsiderable, with no remarkable difference of height by day or night; but that the spring tides rose nine feet perpendicularly in the evening, and scarcely seven in the morning; the difference was uniformly the same on each of the three springs, which happened while he lay at the place, and was apparent for about six or seven days; that is, for about three days before and after the full and change of the moon.

The paper is entitled, "On the Tides in the South Seas." This may have been added by the person who superintended the publication; but whether it was, or was not prefixed by Captain Cook, it evidently points out an intention to generalise, and shows that these observations were considered as a proof of the tides being higher by night than by day, in those seas. It must be confessed that this seems, at first sight, to be not an improbable conclusion, and yet it is one which certainly does not carry complete conviction with it. We do not readily acquiesce in a general conclusion, for which we see no reason, even from analogy; and Captain Cook himself acknowledges that previous to this occurrence, the belief, though common among seamen, had not been confirmed by any thing which had fallen under his own observation. At the conclusion of his paper, he mentions that the wind had prevailed from the S. E. blowing, for the most part, a brisk gale, and rather stronger by day than by night. Now he judged the flood tide to come in that same direction; but how far the height of it was affected by the gale, he does not pretend to determine: indeed it appears that he had no great confidence in this conjecture, although he adds that no other cause had occurred to him which could account for the phenomenon; and he concludes by saying, that he leaves it for others to supply the explanation. It does not appear that this has been done; and some remarks may, therefore, not be without their use. They are not offered as a complete solution of the problem; but if they serve to recall the subject to the attention of the scientific world, they may be the means of producing a more correct investigation from some other writers.

The moon's declination will materially affect the height of the tides. The waters do not rise equally throughout the whole circumference of the same meridian, but they are highest in those parts in which the moon passes the zenith or the nadir, and lowest at 90° from these two points. Hence in the southern hemisphere, when the moon's declination is south, the spring tides will be greatest, which accompany her passage of the meridian; and these will be by night at the full, and by day at the change of the moon. On the contrary, when the moon's declination is north, the spring tides will be greatest; in the southern hemisphere, when they follow about 12 hours after the passage, and these will then be by day at the full, and by night at the time of the new moons. The same might be applied, *mutatis mutandis*, to the northern hemisphere; but it it unnecessary to enter into this part of the detail, as the ship struck in about 15° of south latitude. Now from the Nautical Almanac for 1770, we shall find the following data:

New Moon, June 22, with declination.......	21°	N.
Full Moon, July 7........................	19	S.
New Moon, July 22	17	N.

Hence it would appear, at first sight, that the moon's declination would at once explain the phenomena; but still the difficulty is not removed; for a more particular attention to the paper will show, that it was what is here considered as the day tide, which was observed to rise highest, and not the night tide, as a less attentive perusal of the account would probably lead us to imagine.

We know that in the open ocean, the highest spring tides, at the times of the new and full moon, are in the morning and afternoon about 3 and 15 hours after the moon's passage of the meridian; and though this time hardly suits any other situation, still all the causes which tend to affect the time, act by retarding, and never by accelerating the event. Now Capt. Cook expressly tells us, that at the full and change of the moon, it was high water in the Endeavour river about a quarter after nine; the high tide in the evening must, therefore, have been that which in the open seas had occurred after noon, and which did not arrive at its height in the river till several hours after it would have been high water there, if the moon's attraction could have acted freely and without impediment. This retardation could not be occasioned by the situation which had been chosen in the river, since that was not far from its mouth: neither do the New Hebrides and other islands, which lie to the east of New Holland, appear to form a barrier which could stop the swell of the whole southern ocean for so long a time. The cause, therefore, must be looked for in some other circumstances, and these seem to be the want of a passage for the waters to run off. The east coast of New Holland extends 25°

further to the south; and on the north, there are great obstacles from New Guinea, and that immense collection of islands, which extends almost to the continent. The waters, therefore, are prevented from subsiding towards the west; and as the combined effects of the sun and moon do not raise the surface of the open ocean so much as seven feet, the height of nine feet, as mentioned by Capt. Cook, evidently points out an accumulation.

We have likewise the precise time of high water, and that is a fact which evidently marks the particular tide which we have to consider; and whether we are or are not able to account for the means by which it is produced, still the fact of the retardation is one about which there can be no dispute. This is of considerable importance to the inquiry: it will, therefore, be best to consider the particulars somewhat more in detail, and then to deduce the conclusions which seem justly to be derived from it.

At the time of the new moon of June 22, the moon's declination was about 21° north; and, therefore, when it passed with the sun across the meridian of the place where the Endeavour struck, and which was in south latitude 15° 26′, it passed above 36° from the zenith; the sun likewise had above 23° of north declination, which carried it to nearly 39° of zenith distance. When, however, the sun and moon again passed the plane of the same meridian at midnight, the distance of the moon from the nadir was not 6°, and the distance of the sun was not more than 8°. Hence the waters in the southern ocean must have been raised higher by their attraction at midnight than at noon.

Again at full moon July 7, the moon passed the meridian at midnight with south declination of 19°, which must have carried it within 4° of the zenith; and although the sun's declination was diminished, it still would have passed within 7° of the nadir: whereas at noon, when the sun was on the meridian, it would be 38° from the zenith, and the moon would have been above 34° from the nadir; hence we see again that the tides would have been raised higher at midnight than at noon.

The same will apply to the new moon of July 22 as was laid down for that of June 22; and as the moon's north declination was in this last instance reduced to 17°, it must have passed very near the nadir, and more than compensated for the diminished declination of the sun, the moon's effects on the tides being to that of the sun as five to two.

From the above statement it is clear, that in the open ocean of the South Seas, the tide which followed midnight must, upon each of these three occasions, have been higher than the tides which followed noon. Now this was exactly the reverse of what was observed by Captain Cook in the Endeavour River; for the water there rose higher at the time of the evening than of the morning tide; but these tides occurred at about a quarter after nine; therefore, the high water followed the time at which it would have taken place in the open ocean by above six hours, an interval which would bring the moon and sun into that situation, which would, if there were no impediments, produce low water in the same place. Hence it will be low water out at sea about the time when the tide has arrived at the highest on the coast, and consequently it must then be made to sink by running back towards those parts from which it originally flowed in. It will be seen likewise from this interval of time that the low water out beyond the New Hebrides must have taken place before the tide had come to its height in the Endeavour river. Now a greater depression of the waters must have followed the higher tide, or that which followed midnight, than the lower, or that which took place after noon; and this depression must have taken place out at sea before the time of the greatest accumulation on the coast; hence it would have the greater tendency to diminish, as it were, the sources of that accumulation, and consequently the tide near the land would not have risen so high in the morning as in the evening. For exactly the same reason, the intervening low tide will suffer the greater depression; and Captain Cook says that the low water preceding the highest, fell or receded considerably lower than that which preceded the morning tide.

After much thought on the subject, the above explanation appears to me to be highly probable; and at all events, the discussion will not be without its use. Practical men are often hasty in generalising; and seamen, from the facts which Captain Cook has stated, might be induced to think that what he observed three different times in one place would always occur at least in that part of the world. This I am confident is not the case. I cannot but think that a different aspect of the moon and sun might have reversed the phenomena; but even if these remarks should not be as satisfactory to others as they appear to me, I need not blush to have failed in assigning causes to what even Cook himself was confessedly unable to account for.

Oct. 1, 1818. S.

ARTICLE IX.

Meteorological Table for the Year 1818; *from a Journal kept in Penzance, at the Apartments of the Royal Geological Society of Cornwall.* Communicated to Dr. Thomson by Dr. Forbes, Secretary to the Society.

DEAR SIR, *Penzance, Jan.* 22, 1819.

As we are accustomed in this place to congratulate ourselves on enjoying a milder climate than is possessed by any other town in the kingdom, you will, perhaps, consider an authentic record of the temperature at Penzance, during last year, as meriting a place in your *Annals*. I regret that the account I now send you is not more complete; yet I apprehend, on comparing it with any of the published meteorological journals of last year, it will be found to uphold our claim, not only to a superior degree of mildness of climate, but also to a considerably greater equability of temperature.

Should you think it of sufficient importance to merit publication, it will, I hope, be in my power hereafter to transmit you annually a much more perfect and comprehensive view of our very peculiar Cornish climate.

The height of the thermometer (a common Fahrenheit's) has hitherto been noted only twice a day; viz. between seven and eight in the morning, and at two, p. m.; the following table must not, therefore, be considered as giving the actual maxima and minima observable throughout 24 hours, but only the elevation observed at the particular times stated. In like manner, the *mean* column gives merely the mean of these two observations. I am, dear Sir, with much respect,

Your obedient humble servant,
John Forbes.

1818.	Barometer.				Thermometer.			
	Mean.	Maximum.	Min.	Range.	Mean	Max.	Min.	Range
Jan.	29·81	30·05	28·70	1·35	46°	55	34	21
Feb.	29·64	29·85	28·72	1·13	44	58	34	24
March	29·45	29·30	28·28	1·02	45	58	36	22
April	29·42	30·10	28·90	1·20	51	64	38	26
May	29·63	30·00	29·10	0·90	58	68	48	20
June	29·83	30·10	29·50	0·60	66	78	58	20
July	29·85	30·04	29·50	0·54	67	76	59	17
August	29·81	30·02	29·64	0·38	64	74	58	16
Sept.	29·53	30·04	28·88	1·16	60	70	52	18
Oct.	29·60	30·06	29·10	0·96	58	65	49	16
Nov.	29·57	30·10	29·06	1·04	55	62	48	14
Dec.	29·82	30·30	29·00	1·30	45	58	33	25
Annual Means	29·66	30·01	29·38	1·48	55	65	45	20

N. B. In the account of the thermometer, the fractions are omitted, as of little consequence.

Article X.

Analyses of Books.

Philosophical Transactions of the Royal Society of London, for 1818, Part II.

This part contains the following papers:

I. *On the Parallax of certain fixed Stars.* By the Rev. John

Brinkley, D.D. F.R.S. and Andrews Professor of Astronomy in the University of Dublin.

Our readers are probably aware that an abstract of a letter from Dr. Brinkley to Dr. Maskelyne on the parallax of *α* lyræ was some years ago published in the Philosophical Transactions. Since that time, the author of the letter, in pursuing his observations, has met with apparent motions in several of the fixed stars, the cause of which he was unable to explain, unless by attributing them to parallax. Among these stars, *α* aquilæ exhibited the greatest change of place. The result of these observations has been published in the 12th volume of the Transactions of the Royal Irish Academy. The author there detailed his reasons for supposing that he could not have been misled by any error in the instrument, or in the mode of observing. The attention of Mr. Pond, the Astronomer Royal, was called to this subject by the publications of Dr. Brinkley; and after some years' observations, he was led to doubt the explanation by a parallax being satisfactory. He applied in consequence to the Royal Society, and by their assistance, and the advantage of vicinity to the first artists, he was enabled to put up his fixed telescopes to enable him to bring the question to a final issue.

Mr. Pond considers the observations which he has already made as decisive of the question. This seems likewise to be the opinion of the Royal Academy of Sciences of Paris, since they awarded the Lalande prize to our Astronomer Royal for his observations disproving the opinion that the stars in question have a sensible parallax. The object of the present paper, by Dr. Brinkley, is to show, that Mr. Pond's observations are not sufficient to determine so nice a point. This he does by showing that the unsettled points (as, for example, the allowance for refraction), which must enter into calculations of their quantities, would be more than sufficient to account for all the difference between Mr. Pond's observations and his.

II. *On the Urinary Organs and Secretions of some of the Amphibia.* By John Davy, M.D. F.R.S.—The kidneys of serpents are very large, nearly equal in size to the liver. They are long and narrow, and very lobulated. Like some of the mammalia with conglomerate kidneys, they are destitute of a pelvis. Each lobule sends a small duct to the ureter, which leaves the kidney in two branches. The ureters in general terminate in a single papilla, which is situated in the cloaca between the mouths of the oviducts. It is a little elevated above the surface, and its point is directed towards a receptacle into which the urine enters. The receptacle is a continuation of the intestine; yet it may be considered as distinct both from the rectum and cloaca, with both of which it communicates only by sphincter orifices. The urine is voided occasionally, accompanied by, but never mixed with the fœces. When expelled, it is commonly in a soft state, of a butyraceous consistence, which it loses by exposure to the air, and becomes hard, and like chalk in appearance. The quantity of solid urine secreted by serpents is very great. It was found by Dr. Davy in all cases nearly pure uric acid. The same observation had been previously made in London by Dr. Prout on the excrement of the boa constrictor, and had been communicated by him to Dr. Davy. The urine of lizards was likewise found to be nearly pure uric acid. That of the alligator, besides uric acid, contains a large portion of carbonate and phosphate of lime. The urine of turtles was a liquid containing flakes of uric acid, and holding in solution a little mucus and common salt; but no sensible portion of urea.

III. *On a Mal-conformation of the Uterine System in Women; and on some Physiological Conclusions to be derived from it.* By A. B. Granville, M.D. F.R.S. F.L.S. Physician in Ordinary to H. R. H. the Duke of Clarence.—The subject of this paper was a woman, about 40 years of age, who died at *La Maternitè* in Paris, six or seven days after delivery. She had laboured under an aneurism of the aorta, and an enlargement of the heart. The uterus, four times its usual size, was found to have undergone its full development on the right side only, where it presented the usual pear-like convexity and undulation; while the left exhibited a direct straight line, scarcely half an inch distant from the centre; although more than two inches could be measured from that same point to the outline of the right side. The Fallopian tube and the ovarium, with its surrounding peritonæal folds, were placed as usual on the right side, but could not be found on the left; yet this woman had been the mother of 11 children of both sexes, and had been delivered a few days before her death of twins—a male and a female. This case then destroys the hypothesis of those who laid it down that the male children are derived from one ovarium, and the female children from the other. Perhaps the well-known experiment of Mr. John Hunter, who extirpated one of the ovaria of a sow, which afterwards bore many pigs, no doubt of both sexes (for such an observer would not have failed to notice the singular phenomenon of all the pigs being of one sex, had it existed), may be considered to have already destroyed the supposed evidence in favour of such an hypothesis. But physiologists are obliged to Dr. Granville for recording the present example, as it is an instance more closely applicable to the hypothesis in question than the experiment of John Hunter. Dr. Granville is of opinion that the above case destroys likewise the notion of the possibility of superfœtation. It is not easy to see how it bears upon that question, sufficiently unlikely indeed if we consider it *à priori*, and yet supported by evidence which, if correct, seems to be decisive in its favour; as, for example, a woman bearing at a birth two children; the one white, and the other black.

IV. *New Experiments on some of the Combinations of Phosphorus.* By Sir H. Davy, LL.D. F.R.S. Vice-Pres. R. I.—

Considerable pains have been taken of late years to ascertain exactly the composition of the different compounds of phosphorus; but the subject is attended with so much difficulty, that even the repeated labours of the most eminent chemists of the present day have not been sufficient to elucidate it completely, or to produce full conviction in the minds of those who are practically aware of the difficulties attending these kinds of investigations. Sir H. Davy, whose sagacity and persevering industry place him in the very highest rank of the most eminent chemists that Europe can at present boast of possessing, may be said to have begun the investigation. In a former paper, he made us acquainted with several new compounds of phosphorus, which had not been recognized before, and rectified the notions of chemists about some of the other compounds of phosphorus, which had been previously discovered by Gay-Lussac and Thenard. In his System of Chemistry, he gives us the results of some other experiments; and among others, coincides with Lavoisier, respecting the composition of phosphoric acid, which he considers as a compound of about 1 phosphorus and 1·5 oxygen. Some time after, a paper on the composition of phosphoric acid and the phosphates was given to the world by Berzelius. This paper contained the results of a vast number of experiments, which had occupied the undivided attention of that most indefatigable chemist for several months. About the same period, an abstract of a paper by M. Dulong on the same subject appeared. The paper itself was afterwards published at full length in the third volume of the Memoires d'Arcueil. It contained the discovery of a new acid of phosphorus, to which Dulong gave the name of hypophosphorous acid. It is scarcely necessary to mention my paper on phosphuretted hydrogen gas, published in a preceding volume of the *Annals of Philosophy*. It appears to me to furnish a simpler and more unexceptionable method of determining the composition of the phosphoric and phosphorous acids than any other, and the method that must ultimately decide the question.

The experiments of Berzelius and Dulong differing widely from the former estimates of Davy, he was induced to take up the subject a second time; and the present paper contains the results of his new investigations. After various unsuccessful trials, he found that by putting phosphorus in a glass tube with a narrow mouth, he was enabled, by heating it in a retort filled with oxygen gas, to burn about 10 gr. of it in that elastic fluid, and ascertain the oxygen gas absorbed. From several experiments made in this way, in which from 6 to 10 gr. of phosphorus were burned, he concludes that phosphoric acid is composed of 100 phosphorus + 135 oxygen. From other experiments related in this paper, Sir H. Davy considers himself entitled to conclude, that phosphorous acid contains half the oxygen contained in phosphoric acid.

Let us compare *these experiments* of Davy with the conclusions which I drew from my experiments on phosphuretted hydrogen gas, in a paper published in the *Annals of Philosophy*, viii. 87, or in August, 1816.

1. Phosphuretted hydrogen gas is composed of one volume of hydrogen gas and one volume of vapour of phosphorus condensed into one volume. Hence we can ascertain its composition by subtracting from the specific gravity of this gas the specific gravity of hydrogen gas.

2. The specific gravity of phosphuretted hydrogen gas is 0·9022; that of hydrogen gas is 0·0694. Hence phosphuretted hydrogen gas is composed of

Hydrogen	0·0694 or	1
Phosphorus	0·8328	12

3. I consider it as a compound of one atom of hydrogen and one atom of phosphorus. On this supposition, an atom of phosphorus is 12 times as heavy as an atom of hydrogen; so that if we represent an atom of hydrogen by 0·125, an atom of phosphorus will weigh 1·5 An atom of oxygen weighs 1·000.

4. One volume of phosphuretted hydrogen gas requires for complete combustion either 1 volume or 1·5 volume of oxygen gas.

5. In both of these cases, one half volume of the oxygen goes to the combustion of the hydrogen. The remainder of the oxygen combines with the phosphorus. Thus it appears, that a volume of vapour of phosphorus is capable of combining with half a volume or with one whole volume of oxygen gas.

6. I had already shown, in a paper published in the *Annals of Philosophy*, that one volume of vapour of phosphorus is equivalent to one atom; and that half a volume of oxygen gas is equivalent to an atom.

7. Hence it follows that one atom of phosphorus is capable of combining with one atom of oxygen or with two atoms of oxygen. I concluded, in the paper alluded to, that in the first case, phosphorous acid was formed; in the second case, phosphoric acid. Hence it follows, that the constituents of these two acids is as follows:

	Phos.	Oxygen.	Phos.	Oxygen.
Phosphorous acid	1·5 +	1 or	100 +	66·6
Phosphoric acid	1·5 +	2	100 +	133·3

Now according to Davy, the composition of these acids is as follows:

	Phos.	
Phosphorous acid	100 +	67·5
Phosphoric acid	100 +	135·0

Thus it appears that Davy's experiments and mine do not differ from each other more than one per cent. As his processes were conducted in quite a different way from mine, this very near coincidence induces me to rely upon the results as approaching the truth very nearly. My method was much more susceptible of precision than Davy's. The only part of my experiments in which an error was likely to arise was, in taking the specific gravity of the phosphuretted hydrogen gas; but I do not think the error in that process could be considerable. I am disposed, therefore, to consider the results contained in my paper on phosphuretted hydrogen gas as exhibiting the accurate composition of phosphorous and phosphoric acids.

Soon after the publication of this paper of mine, Dulong's discovery of a new acid of phosphorus, to which he gave the name of *hypophosphorous* acid, became known. This acid contained less oxygen than phosphorous acid. Soon after also, Mr. Dalton, in a paper on phosphuretted hydrogen gas, announced that one volume of it combined with two volumes of oxygen gas. As I was quite sure of the accuracy of my previous proportions, I was led to infer, that a volume of phosphorus is capable of combining with 0·5, 1, and 1·5 volumes of oxygen, or, which is the same thing, 1 atom of phosphorus with 1 atom, 2 atoms, and 3 atoms of oxygen. The most obvious way of accounting for this was to consider the two acids which I had formed as hypophosphorous and phosphorous acids, and to make the new acid of Dalton, the phosphoric acid. This accordingly was the conclusion that I drew in the last edition of my System of Chemistry.

But the new experiments of Davy related in this paper, induce me to revert back again to my original statement; for I think it hardly possible that two sets of experiments, so different from each other as Davy's and mine, could have accorded so nearly as they do, if they were inaccurate.

I must presume, therefore, that Mr. Dalton's result, which I have been myself unable to verify, is either inaccurate, or that he has formed an acid containing more oxygen than the phosphoric.

Dulong's hypophosphorous acid is probably a compound of two atoms of phosphorus and one atom of oxygen. On that supposition, it will consist of

Phosphorus	100·0
Oxygen	33·3

numbers which approach fully as nearly to Dulong's analysis as could be expected, considering the imperfection of the mode which he employed.

These new experiments of Sir H. Davy then possess considerable value. They verify mine, and seem to leave little doubt about the weight of an atom of phosphorus, and the composition of phosphorous and phosphoric acids. But the constitution of phosphoric acid, as it results from the experiments of Davy and my own, does not agree with the constitution of it as resulting from the constituents of the phosphates analyzed by Berzelius. There must, therefore, be an error somewhere; and from the difficulties attending the analysis of the phosphates, and the many anomalies which they present, I am tempted to suspect that new experiments are requisite to make us accurately acquainted with the composition of these bodies.

I do not know whether it be worth while to remark, that Davy has adopted the very same numbers to denote the composition of phosphorous and phosphoric acid as I had done in the paper on phosphuretted hydrogen gas, so often referred to. I represented these acids as composed of

Phosphorous acid, ...	1·5 phosphorus	+	1 oxygen.
Phosphoric acid	1·5		2

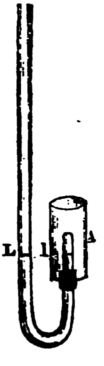
Davy's numbers are,

Phosphorous acid	45 phosphorus	+	30 oxygen
Phosphoric acid	45		60

which are precisely my numbers multiplied each by 30.

V. *New Experimental Researches on some of the leading Doctrines of Caloric; particularly on the Relation between the Elasticity, Temperature, and latent Heat of different Vapours; and on thermometric Admeasurement and Capacity.* By Andrew Ure, M.D.—This paper, which is of considerable length, is divided into three parts.

In the first part, we have a set of experiments to determine the elasticities of the vapour of water, and of other liquids, at different temperatures. The mode of conducting the experiments was ingenious, and it seems capable of more accuracy than any of the previous modes with which I am acquainted. It consisted of a very long glass tube, shut at one end and open at the other, and bent, as in the margin. The glass vessel, A, was cemented round the shut end, and filled with water or oil, which could be heated by means of an Argand's lamp. The liquid to be converted into vapour was put up into the sealed end of the tube; then mercury was poured into the tube till it stood in both legs at the same level, L *l*. The portion above *l* was filled with the vapour of the liquid. The temperature in it was measured by a thermometer, and mercury was poured into the long leg of the syphon till the bulk of the vapour was reduced to what it was when the experiment set out. Thus the elasticity was measured by the height of the column of mercury in the long leg of the syphon.



The following table exhibits the elastic force of the vapour of water at different temperatures, according to Dr. Ure's experiment, represented by the length of the column of mercury in inches which that vapour is capable of supporting.

Temp.	Elasticity.	Temp.	Elasticity.	Temp.	Elasticity.	Temp.	Elasticity.
24°	0·170	165·0°	10·80	250·0°	61·90	292·3°	123·10
32	0·200	170·0	12·05	251·6	63·50	294·0	126·70
40	0·250	175·0	13·55	254·5	66·70	295·6	130·40
50	0·360	180·0	15·16	255·0	67·25	295·0	129·00
55	0·416	185·0	16·90	257·5	69·80	297·1	133·90
60	0·516	190·0	19·00	260·0	72·30	298·8	137·40
65	0·630	195·0	21·10	260·4	72·80	300·0	139·70
70	0·726	200·0	23·60	262·8	75·90	300·6	140·90
75	0·860	205·0	25·90	264·9	77·90	302·0	144·30
80	1·010	210·0	28·88	265·0	78·04	303·8	147·70
85	1·170	212·0	30·00	267·0	81·90	305·0	150·56
90	1·360	216·6	33·40	269·0	84·90	306·8	154·40
95	1·640	220·0	35·54	270·0	86·30	308·0	157·70
100	1 860	221·6	36·70	271·2	88·00	310·0	161·30
105	2·100	225·0	39·11	273·7	91·20	311·4	164·80
110	2·456	226·3	40·10	275·0	93·48	312·0	167·00
115	2·820	230·0	43·10	275·7	94·60	Another exper.	
120	3·300	230·5	43·50	277·9	97·80	312·0	165·5
125	3·830	234·5	46·80	279 5	101·60		
130	4·366	235·0	47·22	280·0	101·90		
135	5·070	238·5	50·30	281·8	104·40		
140	5·770	240·0	51·70	283·8	107·70		
145	6·600	242·0	53·60	285·2	112·20		
150	7·530	245·0	56·34	287·2	114·80		
155	8·500	245·8	57·10	289·0	118·20		
160	9 600	248·5	60·40	290·0	120·15		

Dr. Ure has discovered that if 30 = elastic force of steam at 212° be divided by 1·23, the quotient will exhibit the elastic force of steam at 10° below 212°. This last quotient divided by 1·24 will give the elastic force of steam at 10 below 202°; this last quotient divided by 1·25 will give the elasticity of steam at 10° below 192°, and so on. To obtain the force of steam above 212°, we have only to multiply 30 by 1·23 for the force at 222°; that product multiplied by 1·22 gives the force at 232°; this last product multiplied by 1·21 gives the elasticity at 242°, and so on. Or this empirical formula of Dr. Ure may be represented more generally in this way:

28·9 represent the elasticity of the vapour of water at 210°. Let n represent the number of decades above or below 210° of the degree at which the elasticity of steam is required. Let r = the mean ratio between 210° and the temperature at which the elasticity of steam is required.* Then $\log. 28{\cdot}9 \pm n \,.\, \log. r$ = logarithm of the elasticity required. Above 212° we add, and below 212° we subtract $n \,.\, \log. r$.

Dr. Ure ascertained, by a set of experiments conducted in the same manner, the elasticity of the vapours of alcohol, sulphuric ether, oil of turpentine, and naphtha. The following table exhibits the results which he obtained.

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* By mean ratio is meant the terms 1·23, 1·24, 1·25, &c. or 1·23, 1·22, 1·21, &c. as far as is required, added together, and the sum divided by the number of terms.

Ether.		Alcohol, Sp. Gr. 0·813.				Naphtha.		Oil of Turpentine	
Temp.	Elast.	Temp.	Elast.	Temp.	Elast.	Temp.	Elast.	Temp.	Elast.
34°	6·20	32°	0·40	173·0°	30·00	316°	30·00	304·0°	30·00
44	8·10	40	0·56	178·3	33·50	320	31·70	307·6	32·60
54	10·30	45	0·70	180·0	34·73	325	34·00	310·0	33·50
64	13·00	50	0·86	182·3	36·40	330	36·40	315·0	35·20
74	16·10	55	1·00	185·3	39·90	335	38·96	320·0	37·06
84	20·00	60	1·23	190·0	43·20	340	41·60	322·0	37·80
94	24·70	65	1·49	193·3	46·60	345	44·10	326·0	40·20
104	30·00	70	1·76	196·3	50·10	350	46·86	330·0	42·10
2d	Ether.	75	2·10	200·0	53·00	355	50·20	336·0	45·00
105	30·00	80	2·45	206·0	60·10	360	53·30	340·0	47·30
110	32·54	85	2·93	210·0	65·00	365	56·90	343·0	49·40
115	35·90	90	3·40	214·0	69·30	370	60·70	347·0	51·70
120	39·47	95	3·90	216·0	72·20	372	61·90	350·0	53·80
125	43·24	100	4·50	220·0	78·50	375	64·00	354·0	56·60
130	47·14	105	5·20	225·0	87·50			357·0	58·70
135	51·90	110	6·00	230·0	94·10			360·0	60·80
140	56·90	115	7·10	232·0	97·10			362·0	62·40
145	62·10	120	8·10	236·0	103·60				
150	67·60	125	9·25	238·0	106·90				
155	73·60	130	10·60	240·0	111·24				
160	80·30	135	12·15	244·0	118·20				
165	86·40	140	13·90	247·0	122·10				
170	92·80	145	15·95	248·0	126·10				
175	99·10	150	18·00	249·7	131·40				
180	108·30	155	20·80	250·0	132·30				
185	116·10	160	22·60	252·0	138·60				
190	124·80	165	25·40	254·3	143·70				
195	133·70	170	28·30	258·6	151·60				
				260·0	155·20				
				262·0	161·40				
				264·0	166·10				

Dr. Ure remarks, that the discrepancies in our systems of chemistry respecting the boiling point of oil of turpentine are ludicrous. Dr. Murray makes it 560°, Mr. Dalton under 212°. He himself states the boiling point at 316°. He does not take any notice of my estimate of that point. Had he looked into the first volume of my System of Chemistry (5th edit.), p. 100, he would have found the boiling point of oil of turpentine stated on the authority of an experiment of my own at 314°. The two degrees of difference between his estimate and mine were owing no doubt to the difference between our thermometers. Mine was a standard thermometer made for me by Mr. Creighton. From Mr. Creighton's mode of graduating thermometers, it is obvious that in the higher parts of the scale, the degrees are below the truth. Thus mercury boils, as determined by his thermometers, at 556°: the real boiling point, as determined by Dulong and Petit, is 580°. It is probable that Dr. Ure also employed a thermometer made by Creighton. But it is unlikely that it should be better than mine, as Mr. Creighton was at great pains to make mine as correct as possible, and I paid him a high price for it.

The second topic which Dr. Ure discusses in this paper, is Mr. Dalton's opinion that the common thermometer is an inaccurate measurer of heat, and that mercury and all liquids expand as the square of the temperature, reckoning from the freezing point. It is not necessary to give a particular detail of the facts contained in this part, as Mr. Dalton's opinions on this subject had been already overturned by the experiments of Dulong and Petit.* Dr. Ure's notion that the capacity of bodies for heat diminishes as the temperature increases, is directly contrary to the results of the experiments of Dulong and Petit on the subject. It seems also contrary to analogy in other cases. We know that the capacity of elastic fluids increases as they become rarer, and that the rarest of all the elastic fluids has the greatest capacity. It is reasonable, I think, that this should be the case; for the further the particles of a body are removed from each other, the greater must the quantity of heat be which shall be capable of producing a given effect on it.

In the third part of this paper, Dr. Ure gives us a set of experiments made to determine the latent heat of the vapours of several liquids. He put 200 gr. of the liquid, the latent heat of whose vapour was to be determined into a small retort with a very short neck. The neck entered into a glass globe, which was surrounded by a considerable quantity of water. The latent heat was determined by the degree of heat communicated to the water surrounding the globe. It is obvious that the latent heats determined in this way must be considerably below the truth. The method contrived by Count Rumford seems to me a good deal better. He cooled the water surrounding the globe 4° below the temperature of the room, and continued the distillation till the temperature of the water was exactly 4° above that of the room. During the first half of the process, the water was receiving heat from the air of the room; during the second half, it was giving out heat to the air of the room, and the one quantity must have been exactly counterbalanced by the other. Count Rumford found the latent heat of steam and the vapour of alcohol as follows:

Steam	1040·8°
Vapour of alcohol between	477·0 and 500°

The result of Dr. Ure's experiments is as follows:

Steam	967·000°
Vapour of alcohol	442·000
sulph. ether	302·379

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* The commencement of their important paper will be found in the last number of the *Annals*, p. 118.

Vapour of naphtha	177·870
oil of turpentine	177·870
nitric acid (sp. gr. 1·494)	531·990
ammonia (sp. gr. 0·978)	837·280
vinegar (sp. gr. 1·007)	875·000

Dr. Ure terminates his paper by a very ingenious speculation on the connexion existing between the latent heat, elastic force, and specific gravity of gases or vapours. He conceives that when their tension is the same, the product of their densities into their latent heat will also be the same; or, in other words, that the elasticity is always as the specific gravity multiplied into the latent heat. I have no doubt that we might make considerable progress in the generalization of the properties of elastic fluids by the application of mathematical reasoning; but it would be requisite in the first place to be possessed of a very accurate set of experiments on their expansion, latent heats, specific gravities, &c. Till these are furnished, mathematical reasoning, however ingenious, will serve only to lead us astray. Mr. Dalton in the first volume of his Chemistry, and M. Biot in his late work on Physics, have afforded us some striking examples of the little advantage which results from the application of mathematical reasoning to loose or inaccurate data.

(*To be continued.*)

Article XI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Jan. 21.—A paper, by Dr. T. Young, was read, entitled "Remarks on the Advantage of Multiplied Observations in the Physical Sciences, and on the Density of the Earth." After some observations upon the application of the doctrine of chances to the physical sciences, the author showed that the combination of many different causes of error, each liable to change, has a tendency to diminish the aggregate variation of their joint effect. From calculation he then inferred, that the original conditions of the probability of different errors do not considerably modify the conclusions respecting the accuracy of the mean result, because their effect is included in the magnitude of the mean error from which these conclusions are deduced. He also showed, that the error of the mean arising from this limitation is never likely to be greater than $\frac{6}{7}$ths of the mean of all the errors divided by the square root of the number of observations. The author then proceeded to the application of the doctrine of chances to literary and historical subjects, particularly with respect to the origin of languages and nations. In speaking of the density of the earth, Dr. Y. attempted to show that the general law of compression is quite sufficient to explain the greater density of the interior of the earth, and that this law, which is true for small pressures, in all substances, and universally in elastic fluids, requires some modification for solids and liquids, the resistance in them increasing faster than the density; for no mineral substance, he observed, is sufficiently light and incompressible to afford a sphere as large as the earth, and of the same specific gravity, without such deviation from the general law. A sphere of water or of air would be still more dense, and the moon, if she contained such cavities, would soon have absorbed her atmosphere, if she had ever possessed any.

The paper concluded with some remarks on Euler's formula for the rolling pendulum, in which the perfect accuracy of Laplace's theory, for the length of the convertible pendulum rolling on equal cylinders, was shown.

Jan. 28.—A paper, by Capt. W. J. Webbe, was read, entitled "Memoir of a Survey of the Province of Keemaon." The author stated in this memoir, that from the difficulty of obtaining any thing like an accurate base by route measurement upon the plain, he was induced to consider how far such a base might be accurately deduced from astronomical observations. Having a good reflecting circle, he found that by multiplied observations near the meridian, the latitudes obtained on different days did not vary from one another more than 2″ or 3″. From the difficulties, however, he had to encounter, he was, after all, under the necessity of adopting a proximate primary base, reserving its correction till a future opportunity. In determining the elevations of mountains, he used Mr. Colebrook's formula. The paper concluded with an account of the heights of many of the snowy peaks of the ridge from which the Dnieper, Don, and Volga, descend on the European side, and the Ganges and Indus on the Asiatic; and appended was an extensive catalogue of the latitudes, longitudes, and elevations of places and stations in the province of Keemaon.

At this meeting was also read, a paper, by Professor Aldini, entitled "An Experimental Inquiry upon Gas Light on the Continent, with some Observations upon the Present State of the Illumination of London." After some general remarks, the author suggested, that when coals cannot be obtained, turf may be substituted; also the refuse bark of tan yards, pitch, tar, petroleum, and oil, as now employed by Messrs. Taylor. He also suggested the possibility of employing hydrogen from the decomposition of water for augmenting the quantity of gas.

Feb. 4.—A paper, by W. Bain, Esq. was read, on the dangers to which navigation is exposed by navigators neglecting to make the local attraction on shipboard an element of calculation. The author commenced by making remarks on Capt. Ross's recent observations on the magnetic variation in the northern regions;

and afterwards attempted to point out some of the dangers to which navigators are exposed from inattention to the circumstances producing such local variations.

At this meeting also a paper was read, by W. Scoresby, Jun. Esq. on the anomaly in the variation of the magnetic needle as observed on shipboard. The author began with remarking that the anomalous variation occasioned by the iron of the ship, first pointed out by Capt. Flinders, is now generally admitted. He then proceeded to state his observations upon the subject made during the years 1815 and 1817 upon the coast of Spitzbergen, select tables of which observations were given. To these were added some general inferences upon the subject, deduced at the time of observation, in which it was remarked, that the anomaly is probably greater in large ships of war and merchantmen carrying much iron than in others, though he stated it to be perceptible in all ships, even when iron forms no part of the cargo, especially in high latitudes.

There was likewise read at this meeting an extract of a letter from T. Say, Esq. of Philadelphia, to Dr. Leach, on the subject of the genus Ocythöe. The author commenced by describing a new species of ocythöe. This animal is found occupying the argonauta shell, residing in its last volute. The shell also does not fit the animal, nor is it attached to its body. The author supposed it therefore to be a parasite, and that the animal which forms the argonauta shell may possibly belong to the order pteropoda, though all hitherto observed of this order swim on the surface of the water; for having nothing but fins, they are not calculated to move along the bottom.

There was also read a communication, by L. F. Bastard, of Geneva, entitled "Arithmetical Observations upon the Extraction of Roots." The author offered some remarks upon the extraction of the roots of high powers; and attempted to point out an improved method of effecting that difficult task; but the nature of the communication did not admit of its being read in detail.

Feb. 11.—A paper, by Capt. J. Ross, R. N. was read, on the variation of the compass. The variation of the compass was one of the objects that particularly engaged the author's attention during his late voyage to the Arctic Regions; and he detailed his experiments on this subject in the order in which they were made. From these he concluded, that every ship has a peculiar attraction affecting her compasses, the exact amount of which it is difficult to ascertain. This attraction is not progressive, but irregular, and scarcely admits of general rules; and hence the rules usually given on the subject are not to be depended upon, especially in very high latitudes. In the Isabella, six compasses were found to agree when in the same place; but they all disagreed when removed to different situations between the stern and foremast. Hence the variation of the compass will differ

ting to the place it occupies in the ship. The time of taking the observation also, and the position of the ship's head, modify the variation. The variation is likewise affected by the temperature, density, and humidity of the atmosphere. The direction of the wind and the dip were likewise found to irregularly influence the variation.

Feb. 18.—A paper, by Capt. E. Sabine, was read on the same subject. It was entitled "The Irregularities observed in the Direction of the Compass Needles of H. M. S. Isabella and Alexander, in the late Voyage of Discovery, caused by the Iron contained in them."

LINNÆAN SOCIETY.

Jan. 26.—Mr. Smith's paper, on the Botany of Jersey, Guernsey, Alderney, and Sark, was concluded.

Feb. 2.—A paper, by Mr. John Lindley, entitled, "A Monograph of the Genus *Rosa*," was commenced.

Feb. 16.—The same paper was continued.

GEOLOGICAL SOCIETY.

Dec. 18.—A communication was read from Thomas Robinson, Esq. of Morley Park Iron Works, near Belper, Derbyshire, on a tree, apparently oak, found in these works.

As the miners were sinking a pit for the purpose of obtaining iron ore, they discovered a tree, apparently oak, in an erect position, its bottom standing below the third measure of iron stone; its length was about six feet, and its diameter 10 to 14 inches; and its substance dark coloured and mouldering; its position, and the unbroken appearance of the beds it traversed, seem to countenance an idea, that it grew there previously to the deposition of the beds surrounding it.

A communication was received from the Rev. William Buckland, B.D. F.R.S. V.P.G.S. and Reader in Geology and Mineralogy in the University of Oxford, and the Rev. W. D. Conybeare, A.M. M.G.S. "On the Geological Structure of the South Western Coal District, and on the Relations of the Deposites by which it is partially covered."

Jan. 1, 1819.—The reading of Mr. Buckland's paper, on the South Western Coal District, was concluded.

This paper is understood to be introductory to a series of communications on this district, which appears generally to consist of two principal formations.

The first reposes on the transition rocks, and includes the independent coal formation of the Wernerian school.

The second consists of more recent horizontal deposites, lying unconformably on the transverse edges of the first formation, and partially filling the valleys and low grounds between the ridges constituted by them.

The first formation consists of the following beds, beginning with the lowest.

1. Beds of transition limestone and imperfect slate which the

author supposes of the same era with those which occur near Malvern, and at Ludlow and Wenlock-edge, and considers as the upper members of the graywacke series, and a link between the transition slate-rocks and succeeding formations.

2. Old red sandstone.
3. Mountain limestone.
4. Coal measures.

All the beds of this series are highly inclined, and thrown by their undulations into various basins, each of which contains a succession of coal-measures surrounded by bands, formed by the outcrop of the subjacent beds of mountain limestone and old red sandstone.

The principal of these basins are; 1. That of Somerset and South Gloucester, including the collieries of Mendip, Kingswood, and Sodbury.

2. That of the forest of Dean.
3. That of South Wales.

The second formation, beginning with the lowest beds, consists of

1. Calcario-magnesian conglomerate, and magnesian limestone.
2. Newer red sandstone and red marl.
3. Lias.
4. Oolite, which rises to a greater elevation than the three preceding beds, and skirts the eastern border of the district under consideration.

Besides these regular formations, two whin-dykes traverse the north border of the Somerset and Gloucester basin, near Berkeley, extending north and south nearly parallel to each other for about two miles, and cutting the transition limestone and old red sandstone. At one point, called Woodford, one of these dykes has been said to contain organic remains; but these have been found only in portions of the limestone, entangled, and partially enveloped by the sides of the dyke. This trap contains agates, prehnite, sulphate of strontian, carbonate of lime, green earth, and ferriferous magnesian carbonate of lime: the two latter abound in the amygdaloidal varieties at Woodford. In one spot near its south extremity, the dyke becomes columnar.

This paper contains some precise observations of the angles of inclination and direction of the different strata, which, though of little importance when taken singly, possess considerable value in reference to the structure of an extensive district.

A note points out the recurrence of magnesian limestone in all the formations from primitive dolomite upwards through transition limestone, oolite, and chalk, and also that it exists in the London clay. An appendix contains a list of previous works in which accounts of the district under examination may be found, and the authors have given a very brief but useful abstract of these contents.

A paper was read, on the rock of Gibraltar, by Thomas Kent, Esq. communicated through William Cosens, Esq. both of Gibraltar.

The rock is a mass of limestone, whose greatest height is about 1,440 feet, and its base about 2,200 feet, in its longer diameter. The small rock on which the Devil's Tower is built, appears to be a fragment fallen from it: the edge of the summit is in some places so sharp that a person cannot stand upon it. Part of the rock appears to have been much broken and dislocated, and in the intervals between the fragments, as well as in a cavern in the side of the east cliff, bones have been found incrusted with stalactitic carbonate of lime. The hills near St. Roque, reaching for a distance of several miles into Spain, contain large oyster and cockle, and other shells; but the author has not examined the beds.

The ancient city of Carteia was built of the stone from these hills.

ARTICLE XII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Durham Coal Field.*

We understand that it is in contemplation at present to open the Coal Field of Durham into Yorkshire. In the mean time, a bill is to be brought into Parliament to carry a rail-way from Bishop Auckland to Darlington and Stockton. Mr. Stevenson, of Edinburgh, one of the most accomplished engineers of this country, has been called by the committee of subscribers to give an opinion as to the best line. The work is estimated at about 120,000*l.*, a great part of which is already subscribed.

II. *Melting Points of Bismuth, Tin, and Lead.*

Mr. Creighton, of Glasgow, who has been long celebrated for the beauty and accuracy of the philosophical instruments made by him, and who has consecrated the evening of his life, in a great measure, to the manufacture of thermometers, has made some remarks on the boiling points of bismuth, tin, and lead, which deserve to be better known than they seem to be at present. He announced his determination of the melting points of these metals in an early volume of the Philosophical Magazine; but whether the facts to which I wish at present to draw the attention of the chemical reader were noticed by him in his original paper, I do not recollect, as I have not the early volumes of that work at present by me. If they were noticed, they seem not to have attracted the attention of chemists; for I am not aware of any chemical book in which they are mentioned.

The melting points of these metals, as determined by Mr. Creighton, are as follows:

Lead	612°
Bismuth	476
Tin	442

Now the curious circumstance attending these metals is this: When they cool down to the melting point, bismuth instantly sinks 8°, and immediately rises again; tin instantly sinks 4°, and immediately rises again; while lead undergoes no change whatever, but remains stationary at 612° till the whole is congealed.

It is well known that water in certain circumstances may be sunk down considerably below the freezing point without congealing; but the instant it begins to congeal, it rises again to 32°, at which it remains stationary till the whole is converted into ice. The subsidence of the bismuth and the tin is obviously analogous to that of the water, and the subsequent rise is doubtless owing to the commencement of congelation in these metals. The curious circumstance is, that each sinks a definite number of degrees, and that lead does not sink at all. I conceive that these phenomena depend upon the latent heat of these liquid bodies. When water is cooled down below its freezing point, it gives out a portion of its latent heat. The evolution of the latent heat, as it congeals, raises the temperature to 32°, and keeps it at that point till the whole water is converted into ice. Bismuth and tin, in like manner, may be cooled down several degrees below their point of congelation, and the heat they give out is a portion of their latent heat. When they begin to congeal, that portion which becomes solid gives out the whole of its latent heat, and this evolution keeps up the temperature at the melting point till the whole has congealed. But the latent heat of lead is much smaller than that of the other two metals. It seems this metal is incapable of parting with a portion of its latent heat. The whole of it escapes at once in proportion as the metal congeals: consequently the thermometer must remain stationary. The latent heat of these three metals, according to the experiments of Dr. Irvine, is as follows:

Bismuth	550°	It loses $\frac{1}{70}$ of its latent heat.
Tin	500	It loses $\frac{1}{125}$.
Lead	162	

III. *Japan Copper.*

Bergman states the specific gravity of copper at 9·3243 (De Niccolo, Opusc. ii. 263). Cronstadt states the specific gravity of Japan copper to be 9·000. I have never myself been able to meet with copper of even so high a specific gravity as that given by Cronstedt, though I have examined the purest copper used in this country for alloying gold, and in which I could detect no sensible quantity of any foreign ingredient. I was naturally anxious on that account to take the specific gravity of the best kinds of Japan copper. This I have been enabled to do by the kindness of Professor Jameson, who got a piece of Japan copper, said to be of the very best quality, from a gentleman who had been in the habit of dealing largely in that article of commerce in India, and had himself (for he was the captain of a ship) carried it from Japan to India in great quantities. I found its specific gravity only 8·434, and hence, I think, we may conclude, that the number assigned by Cronstedt for the specific gravity of copper is above the truth. Bergman's number, *à fortiori*, is also in excess.

IV. *Measurement of an Arc of the Meridian in India.*

Many of our readers are probably aware that a trigonometrical survey of India has been going on for a good many years, at the expense of the British government in that country, and under the superintendence of British officers well qualified for performing a task of that kind. Lieut.-Col. William Lambton, F.R.S. of the 33d reg. of foot, took the opportunity of this survey to measure, at different times, an arc of the meridian from north latitude 8° 9′ 38″ to north latitude 18° 3′ 23·6″, being an amplitude of 9° 53′ 45″, the longest single arch that has ever been measured on the surface of the globe. The full details of this great measurement are partly contained in the 12th volume of the Asiatic Researches; and will be partly inserted in the 13th volume of that work, which will not probably be published for these three or four years. Col. Lambton has inserted an abstract of the principal results into a paper, which has been published in the second part of the Philosophical Transactions for 1818. From that paper I shall take a few of the facts which are most likely to be generally interesting to European readers.

1. The mean length of a degree due to latitude 9° 24′ 44″ in fathoms, is .... 60472·83
The mean length of ditto due to lat. 12° 2′ 55″, is .. 60487·56
The mean length of ditto due to lat. 16° 34′ 42″, is 60512·78

Thus we see that these measurements show the degree lengthening as we advance towards the pole. In this respect, they agree with all preceding observations, which demonstrate that the polar axis of the earth is shorter than the equatorial.

2. Col. Lambton has shown by a comparison of his measurements with the length of a degree as determined in France, in England, and in Sweden, that the compression at the poles amounts to $\frac{1}{310}$ of the length of the axis.

The comparison of the Indian measurement with the French measurement, gives $\frac{1}{309.13}$ for the compression.

The comparison of the Indian measurement with the English measurement gives $\frac{1}{313.54}$.

While the comparison of the Indian with the Swedish measurement gives $\frac{1}{307.19}$ for the compression.

The mean of these three comparisons gives $\frac{1}{309.96}$, or almost $\frac{1}{310}$ for the compression at the poles.

3. From the preceding compression of $\frac{1}{310}$, Col. Lambton has calculated the length of a degree of latitude from the equator to the pole. The following table exhibits the result of this calculation. The last column of the table gives the length of the degree of longitude at the latitude indicated in the first column of the table.

Lat.	Degrees on the meridian.	Degrees on the perpendicular.	Degrees of longitude.
0	60459·2	60848·0	60848·0
3	60460·8	60848·4	60765·0
6	60465·6	60850·1	60516·8
9	60473·5	60852·8	60103·6
12	60484·5	60856·5	59526·7
15	60498·4	60861·1	58787·3
18	60515·1	60866·7	57887·7
21	60534·3	60873·2	56830·0
24	60556·0	60880·5	55628·1
27	60579·8	60888·5	54252·0
30	60605·5	60897·1	52738·4
33	60632·7	60906·2	51080·2
36	60661·3	60915·8	49281·9
39	60690·8	60925·7	47348·2
42	60721·3	60935·7	45284·0
45	60751·8	60946·1	43095·4
48	60782·3	60956·4	40787·8
51	60812·5	60966·5	38367·5
54	60842·1	60976·5	35841·1
57	60870·7	60986·1	33215·4
60	60898·0	60995·2	30497·6
63	60923·7	61003·8	27695·2
66	60947·5	61011·8	24815·7
69	60969·1	61018·9	21867·2
72	60988·3	61025·6	18857·9
75	61005·1	61031·0	15796·0
78	61018·9	61035·8	12690·1
81	61029·9	61039·5	9548·7
84	61037·8	61042·1	6380·6
87	61042·6	61043·7	3194·8
90	61044·3	61044·3	——

4. From this table it appears, that the length of a degree of latitude at the poles is .... 68·704 English miles
At lat. 45°.. .... 69·030
At lat. 51° .... 69·105
At lat. 90° .... 69·368

So that the mean length and degree of latitude is almost exactly 69 miles and $\frac{1}{10}$th of a mile. Of consequence, the common estimate of 69 miles and a half to a degree is very erroneous.

V. *Protoxide of Copper.*

About two years ago, I received from Mr. Mushet, of the Mint, part of a mass of copper, which had been for a considerable time exposed to heat in one of the melting furnaces at the Mint, of which he has the superintendence. The copper was changed into a red, granular, brittle mass, very similar in appearance to red copper ore. Grains of copper were interspersed through it in very small quantity. On reducing a portion of the specimen to powder, and pouring muriatic acid over it in a retort, I very speedily obtained a dark coloured opaque solution, quite similar to what is obtained when muriatic acid is poured upon a mixture of equal weights of fine powdered copper and black oxide of copper. This solution is known to consist of protoxide of copper dissolved in muriatic acid. When dropped into water, a white powder falls, consisting of protohydrate of copper. When dropped into a solution of potash, a yellow-coloured precipitate falls, which consists of protoxide of copper. The muriatic acid solution of Mr. Mushet's specimen exhibited exactly these appearances, and proved to be a pure solution of protoxide of copper in muriatic acid. Here then we have an instance of copper converted by heat into protoxide. It is the first example of the kind which I have yet met with; and on that account deserves the particular attention of chemists. All such accidental conversions of metals into unusual oxides ought to be carefully recorded.

I made an analysis of a portion of this curious specimen, which is not, however, to be considered as rigidly exact; for a very accurate numerical statement of such a mixture does not seem to lead to any very useful consequence. I found the constituents of 100 gr. of the specimen as follows:

Protoxide of copper	43·8
Protoxide of iron.	26·2
Silica (not quite free from iron and copper)	30·0
	100·0

⁂ The mass of copper, above-mentioned, was obtained from the bottom of a furnace used for the melting of copper. The bottom of the furnace is from 9 to 12 inches thick, formed with a round grained sand, such as glass grinders use. This mass of sand vitrifies, and becomes extremely hard, but porous; so that in the melting of copper, grains of the metal will insert themselves. The great proportion, however, in the mass, exists in a state of oxide; and by the continued use of the furnace, the greater proportion of the sand will be regularly converted into a red coloured copper ore. In the instance in question, the mass of sand converted into this red coloured ore exceeded six inches. R. M.

VI. *Fall of Stones from the Atmosphere.*

Among the very minute historical details of the falls of stony bodies from the atmosphere, from the earliest ages down to our own time, which have been successively published by Dr. Chladni, I do not find the following. The attention of meteorologists has been drawn to it by Sig. Domenico Paoli, in a letter published in Brugnatelli's Journal for July and August, 1818. The passage quoted is taken from the fifth chapter of the first book of a work published by Camillo Leonardi, in the year 1502. The title of the book is Speculum Lapidum. Leonardi was an inhabitant of Pesaro, in Italy, where his book was published. The passage is as follows:

"Et non solum in locis his dictis lapides generantur, verum etiam et in aëre, sicut habetur a philosophis, et maxime ab illo summo philosopho, ac nostris temporibus monarca, preceptore meo Domino Gaetano de Fienis, in comento metaurorum, in fine secundi tractatus libri tertii, qui dicit: Lapides generari possunt in aëre, cum exhalatio habet partes grossas terreas admixtas cum humiditate grossa viscosa. Et resolutis partibus magis subtilibus, et terrestribus condensatis a calido, fit lapis, qui ratione suæ gravitatis ad terram descendit. Nostris temporibus, in partibus Lombardiæ lapis magnæ quantitatis ex nubibus cecidit."

VII. *Blue Glass from Iron.*

It is pretty well known that the ancients were acquainted with a method of giving a fine blue colour to glass by means of iron. This method has been lost, probably because cobalt, the tinging substance used by the moderns, is much easier and much more certain of answering the object intended. Iron, however, if we are to judge from ultramarine, which owes its blue colour to iron, is capable of communicating a more beautiful colour to glass than cobalt; besides, cobalt is a very scarce metal, and sells at a high price; while iron is the most abundant and the cheapest of all known metals. On these accounts, it would be an object of considerable interest to painters, glass makers, and potters, if the ancient art could be again recovered. M. Pagot Descharmes has made a number of trials, and has made known the results which he obtained in a paper published in the Journal de Physique, for July, 1818. From the imperfect experiments which he describes in this paper, I am tempted to suspect that the *chloride of iron* is the substance possessed of this desirable property. Probably successful results might be obtained by adding chloride of iron to glass already in fusion. It would be an object worth the while of our Staffordshire potters to try the properties of chloride of iron and some other metallic chlorides as paints, either mixed with glass in the proportions that suited best, or perhaps mixed with their common enamels. There is every reason to expect that these chlorides would communicate colours different from the oxides of the same metals. If colours could be made from them for the use of the painters by uniting them with silica, as is the case with ultramarine, such colours would be much more valuable than those at present in use; because they would not be liable to undergo alterations from the action of the atmosphere, or the light of the sun. Our painters at present make use of colours possessed of so little permanency that the picture is scarcely calculated to outlive the artist.

VIII. *Fusion of Platinum.*

It is said that M. Prechtel, Director of the Polytechnical Institute at Vienna, has succeeded in fusing platinum by means of a very violent heat in very refractory crucibles. The greatest degree of heat which he has produced may be estimated at 180° Wedgewood. When platinum is thus fused, its specific gravity is reduced to 17⅔. It may be scratched by a knife. It may be readily beat out under the blows of the hammer, and may be easily divided by the saw, like copper. When heated to redness and struck with a hammer, it scales off, and exhibits a granular fracture, similar to that of cast-iron. This leads to the opinion that the platinum crystallizes during its solidification. Crude platinum does not fuse at so low a heat as pure platinum.—(Gilbert's Annalen, Jan. 1818.)

IX. *Formation of the Vegetable Epidermis.*

Grew and Malpighi were of opinion that the epidermis of plants is merely a scurf formed upon the parenchyma of the bark by the action of the air. Mirbel has lately supported the same doctrine, and endeavoured to obviate the objections that naturally rise in one's mind when such an opinion is advanced. But Mr. Keith has shown that some of the most formidable objections of all have not been noticed by him. If the vegetable epidermis were merely the result of the action of the air upon the parenchyma, it would follow that the epidermis would never be formed till the part were actually exposed to the action of the air. But this is not the case. If we strip a rose bud, or any other flower bud of its covering, we shall find that every petal is covered with just as perfect an epidermis as those parts of the plant which have been exposed to the air. When the epidermis of the leaves or petals is rubbed off, it is never renewed. When the epidermis of the *stems* of woody plants is rubbed off, it is renewed more speedily and more perfectly when the part is covered up from the action of the air than when it is exposed to that action. These facts, stated by Mr. Keith, seem to leave no doubt that the use of the epidermis in plants is the very same as in animals: that it is formed for the express purpose of protecting the parts below it, and that the analogy between the animal and vegetable epidermis is complete.—(Linnæan Trans. xii. 6.)

X. *Method of procuring Meconic Acid.*

The infusion of opium, from which the morphia had been precipitated by means of ammonia, was evaporated to the consistence of a syrup, and left in a state of rest; but no crystals would form in it.* It was then diluted with 16 ounces of water, and mixed with one ounce of caustic ammonia. As no precipitate appeared after the interval of an hour, the liquid was heated to drive off the excess of ammonia. When heated to the temperature of 122°, it became muddy, and 15⅓ gr. of impure morphia were precipitated.

The liquid being freed from this precipitate and from the excess of ammonia, muriate of barytes was poured into it as long as any precipitate fell. The precipitate, being collected and dried, weighed seven drams, and was Sertürner's meconate of barytes. To obtain the meconic acid from this salt, M. Choulant triturated it in a mortar, with its own weight of glassy boracic acid. This mixture being put into a small glass flask, which was surrounded with sand in a sand pot in the usual manner, and the heat being gradually raised, the *meconic acid* sublimed in the state of fine white scales, or plates.

XI. *Properties of Meconic Acid.*

It has a strong sour taste, which leaves behind it an impression of bitterness.

It dissolves readily in water, alcohol, and ether.

It reddens the greater number of vegetable blues, and changes the solutions of iron to a *cherry-red* colour. When these solutions are heated, the iron is precipitated in the state of protoxide.

The meconiates, examined by Choulant, are the following:

(1.) *Meconiate of Potash.*—It crystallizes in four-sided tables, is soluble in twice its weight of water, and is composed of

Meconic acid	27	2·7
Potash	60	6·0
Water	13	
	100	

It is destroyed by heat.

(2.) *Meconiate of Soda.*—Crystallizes in soft prisms. Soluble in five times its weight of water. Seems to effloresce. Decomposed by heat. Its constituents are,

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* See the notices on Morphia in the last Number, p. 159.

Acid	32	3·2
Soda	40	4·0
Water	28	
	100	

(3.) *Meconiate of Ammonia.*—Crystallizes in star-form needles, which, when sublimed, lose their water of crystallization, and assume the shape of scales. The crystals are soluble in 1½ their weight of water, and are composed of

Acid	40	2·024
Ammonia	42	2·125
Water	18	
	100	

If two parts of sal ammoniac be triturated with three parts of meconiate of barytes, and heat be applied to the mixture, meconiate of ammonia is sublimed, and muriate of barytes remains.

(4.) *Meconiate of Lime.*—Crystallizes in prisms. Soluble in eight times its weight of water. Its constituents are,

Acid	34	2·934
Lime	42	3·625
Water	24	
	100	

Choulant not having succeeded in obtaining meconiate of barytes in crystals, did not attempt to analyze it.

XII. *On the Equivalent Number for Meconic Acid.*

The numbers annexed to the analyses of Choulant, represent the equivalent numbers for the bases and meconic acid in each analysis. We see from them that the results obtained by this chemist are far from correct; for the equivalent number for meconic acid varies in each analysis. These numbers are as follows:

From meconate of potash	2·700
soda	3·200
ammonia	2·024
lime	2·934

The mean deduced from these four salts gives us 2·714 for the weight of an atom of meconic acid. The number 2·75, therefore, may be considered as an approximation; but probably not a very near one. 2·75 represents the weight of an atom of carbonic acid. But it would be premature to speculate on this subject till we are in possession of more accurate analyses of the meconiates.

XIII. *Celestine from Fassa, in the Tyrol.*

Celestine, or sulphate of strontian, was first discovered in the neighbourhood of Bristol; afterwards in Pennsylvania; then in Sicily; and more lately in different parts of Germany, France, and England. Dr. Rodolph Brande has published a very elaborate analysis of the variety of this mineral, which occurs at Fassa, in the Tyrol.

Its colour is yellowish white.

The fracture radiated, with a threefold cleavage.

Lustre, pearly, approaching vitreous.

Translucent in the edges.

Specific gravity, 3·769.

According to the analysis of Dr. Brande its constituents are as follows:

Sulphate of strontian	92·1454
Sulphate of lime	1·3333
Sulphate of barytes	1·8750
Carbonate of strontian	1·6470
Carbonate of lime	0·5000
Silica	1·0000
Oxide of iron	0·5000
	99·0007

(Schweigger's Journ. xxi. 177.)

XIV. *Wodanium.*

In our number for January, we announced the discovery of a new metal by Lampadius, which he has distinguished by the name of Wodanium: we shall now translate the account of this new metal which Lampadius has himself published.

"Our venerable mine superintendent, Von Trebra, has had in his possession for several years a metallic mineral from Topschau, in Hungary, under the name of a cobalt ore. But as it gives no blue colour, I got it from him in order to make some further trials on it. I could detect in it no cobalt; but found in it 20 per cent. of a new metal united with sulphur, arsenic, iron, and nickel.

"This metal has a bronze yellow colour, similar to that of cobalt glance; and its specific gravity is 11·470.

"It is malleable; its fracture is hackly; it has the hardness of fluor spar; and is strongly attracted by the magnet.

"It is not tarnished by exposure to the atmosphere at the common temperature; but when heated, it is converted into a black oxide.

"The solution of this metal in acids is colourless; or at least has only a slight wine-yellow tinge. Its hydrated carbonate is likewise white. The hydrate of it precipitated by caustic ammonia is indigo blue.

"Neither the alkaline phosphates nor arseniates occasion any precipitate, when dropped into a saturated solution of this metal in an acid: neither is any precipitate produced by the infusion of nutgalls. A plate of zinc throws down a black metallic powder from the solution of this metal in muriatic acid. Prussiate of potash throws down a pearl-grey precipitate, &c.

"Nitric acid dissolves with facility both the metal and its oxide, and the solution yields colourless needle-form crystals, which readily dissolve in water.

"As the names of the planets have been already all applied to newly discovered metals, I have, in imitation of Berzelius, had recourse to the old German mythology, and give the metal the provisional name of *Wodan,* or *Wodanium.* My worthy friend Breithaupt classes the mineral that contains this new metal among the pyrites, and gives it the name of Wodan pyrites (*Wodan-kies*). He gives the following description of this mineral.

"Wodan pyrites has the metallic lustre, and is shining or glistening.

"Its colour is dark tin-white, passing into grey, or into brown.

"Hitherto it has occurred only massive; and in that state it is full of cavities.

"The fracture is uneven, and either small or great granular. Fragments indeterminate angular, with edges not peculiarly sharp.

"Harder than fluor spar; but softer than apatite.

"Brittle. Easily frangible.

"Specific gravity, 5·192."

Lampadius informs us, in the letter of which the preceding paragraphs contain the translation, that he intends to publish a full account of the new metal and its ore in the Transactions of the Mineralogical Society of Dresden.—(Gilbert's Annalen de Physik, lx. 99, for September, 1818.)

XV. *Potters' Clay.*

Near the Halkin Hills, in Flintshire, and within four miles of the sea, some miners discovered, about two years ago, a vast bed, of a substance said to be adapted for the manufacturing of earthen ware without the addition of any other material. It lies immediately under a stiff red clay, and coals abound in the neighbourhood. The miners and Mr. Bishop, of Stafford, have taken a lease of the ground from the proprietor, Lord Grosvenor.

A specimen of the substance has been brought to London, but has not yet been analyzed. A more full account of it will probably be given in a future number.

Near the same place also has been found a hollow siliceous rock, abounding in organic impressions, which has been supposed likely to become a substitute for burrstone, but it appears to be too brittle for this purpose.

XVI. *Meteorological Table. Extracted from the Register kept at Kinfauns Castle, N. Britain. Lat.* 56° 23′ 30″. *Above the Level of the Sea* 129 *feet.*

1818.	Morning, 8 o'clock. *Mean height of*		Even., 10 o'clock. *Mean height of*		Mean temp. by Six's Ther.	Depth of Rain.	No. of days.	
	Barom.	Ther.	Barom.	Ther.		In. 100	Rain or Snow.	Fair
Jan.	29·447	36·970	29·457	35·322	37·129	2·45	22	9
Feb.	29·464	34·321	29·453	34·643	35·857	0·86	14	14
March	29·302	35·419	29·345	36·193	37·516	1·62	18	13
April	29·732	39·333	29·733	38·833	41·266	1·03	7	23
May	29·857	49·290	29·858	48·486	52·613	1·67	15	16
June	29·869	57·430	29·839	55·909	59·033	1·34	10	20
July	29·912	59·774	29·905	58·161	60·355	3·20	15	16
Aug.	29·960	55·709	29·942	54·548	56·903	0·70	7	24
Sept.	29·628	52·136	29·611	50·466	53·100	1·99	14	16
Oct.	29·711	51·032	29·709	50·193	52·387	1·40	12	19
Nov.	29·681	46·100	29·681	47·100	47·500	2·22	17	13
Dec.	29·908	38·451	29·917	38·419	39·226	1·41	9	22
Aver. of year.	29·706	46·330	29·703	45·688	47·740	19·89	160	205

ANNUAL RESULTS.

MORNING.

Barometer. Observations.	Wind.		Thermometer.	Wind.	
Highest, April 3	N W	30·60	July 17	S	69°
Lowest, March 5	S W	28 12	July 3 and 4	W	21°

EVENING.

Highest, April 2	N W	30·58	July 16	W	68°
Lowest, March 4	E	28·44	Feb. 22	W	19°

Weather.	*Days.*
Fair	205
Rain or Snow	160
	365

Wind.	*Time.*
N and N E	19
E and S E	132
S and S W	93
W and N W	121
	365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, February 5, Wind W.	17°
Hottest, June 11 and 12, Wind W.	77
Mean temperature for 1818	47·740°

Result of three Rain Gauges.	*In.* 100
No. 1. On a conical detached hill above the level of the sea 600 feet	31·10
No. 2. Centre of the garden, 20 feet	28·07
No. 3. Kinfauns Castle, 129 feet	19·89
Mean of the three gauges	26·35

XVII. *Register of the Weather at New Malton, in Yorkshire.*

Sept.—Mean pressure of barometer, 29·580; max. 30·10; min. 29·05; range, 1·05 in.; spaces described by the curve, 08 in.; number of changes, 13.—Mean temperature, 55·080°; ax. 73°; min. 39°; range, 34°.—Amount of rain, 3·24 in. et days, 13. Prevailing winds, S. and W. N. 1; N.E. 1; . 2; S.E. 5; S. 8; S.W. 4; W. 2; N.W. 6; var. 1; brisk nds, 7; boisterous, 4.

Oct.—Mean pressure of barometer, 29·640; max. 30·12; in. 28·99; range, 1·13 in.; spaces described by the curve, 55 in.; number of changes, 13.—Mean temperature, 52·300°; ax. 65°; min. 39°; range, 26°.—Amount of rain, 2·37 in. et days, 9. Prevailing wind, S. N.E. 2; E. 5: S.E. 4; 12; S.W. 4; W. 3; N.W. 1; brisk winds, 4; boisterous, 1.

Nov.—Mean pressure of barometer, 29·596; max. 30·10; in. 29·10; range, 1·00 in.; spaces described by the curve, 68 in.; number of changes, 12.—Mean temperature, 46·766°; ax. 57°; min. 34°; range, 23°.—Amount of rain, 3·10 in. et days, 10; brisk winds, 1; boisterous, 1. Prevailing winds, N. 2; N.E. 3; E. 1; S.E. 9; S. 4; S.W. 8: W. 2; var. 1.

Dec.—Mean pressure of barometer, 29·860; max. 30·41; in. 29·10; range, 1·31 in; spaces described by the curve, 45 in.; number of changes, 16.—Mean temperature, 35·903°; ax. 55°; min. 24°; range, 31°.—Amount of rain, 1·00 in.; et days, 5. Winds, var. N. 8; S.E. 4; S. 3; S.W. 7; . 6; N.W. 3; boisterous winds, 1.

For the Year, 1818.—Mean pressure of barometer, 29·647; ax. 30·49; min. 27·85; range, 2·64 in.; spaces described by e curve, 79·00 in.; number of changes, 177.—Mean temperture, 48·284°; max. 88°; min. 23°; range, 65°.—Amount of in, 32·47 in.; wet days, 103; snowy, 23; haily, 1. Winds, .W. and W. N. 46; E. 33; N.E. 28; S.E. 39; S. 56; .W. 84; W. 50; N.W. 15; var. 14; brisk winds, 55; boiserous, 27.

New Malton, Jan. 7, 1819. J. S.

XVIII. *Death of Professor Luigi Brugnatelli.*

Professor Luigi Brugnatelli was born in Pavia, in 1761. He as appointed assistant to Prof. Scopoli, in 1787, and succeeded im as Professor of Chemistry in the University of Pavia, in 796, which situation he held till his death on the 24th of Oct. 818. He was not the editor of the *Giornale di Fisica,* which conducted by Dr. Gaspar Brugnatelli.

ARTICLE XIII.

Magnetical, and Meteorological Observations.

By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 42″ North. Longitude West in time 1′ 20·7″.

Magnetical Observations, 1819. — *Variation West.*

Month.	Morning Observ.					Noon Observ.					Evening Observ.	
	Hour.		Variation.			Hour.		Variation.			Hour.	Variation
Jan. 1	8h	45′	24°	37′	55″	—h	—′	—°	—′	—′	Owing to the shortness of the days, evening observation discontinued.	
2	8	45	24	35	48	1	15	24	38	30		
3	8	40	24	37	18	1	20	24	40	44		
4	8	45	24	39	26	1	55	24	40	50		
5	8	45	24	41	00	1	15	24	41	17		
6	8	50	24	38	10	1	20	24	41	52		
7	8	35	24	36	45	1	20	24	40	59		
8	8	45	24	34	43	1	10	24	41	28		
9	8	50	24	34	29	1	55	24	39	56		
10	8	40	24	35	27	1	20	24	40	08		
11	8	40	24	34	57	1	15	24	40	54		
12	8	40	24	35	24	1	20	24	41	25		
13	8	35	24	35	47	1	10	24	40	55		
14	8	50	24	35	33	1	15	24	40	15		
15	8	40	24	35	15	1	15	24	38	58		
16	8	40	24	35	57	1	15	24	37	50		
17	8	45	24	35	50	1	20	24	38	11		
18	8	45	24	32	20	1	55	24	33	56		
19	8	40	24	31	59	1	35	24	37	14		
20	6	40	24	35	38	1	20	24	38	55		
21	8	30	24	34	22	1	20	24	37	08		
22	8	35	24	34	36	1	15	24	40	17		
23	8	40	24	34	06	1	20	24	40	46		
24	8	35	24	35	21	1	15	24	41	46		
25	8	35	24	36	39	—	—	—	—	—		
26	8	40	24	35	59	1	15	24	40	42		
27	8	45	24	35	14	1	15	24	39	01		
28	8	45	24	35	31	1	15	24	41	10		
29	8	40	24	35	36	1	20	24	41	25		
30	—	—	—	—	—	—	—	—	—	—		
31	8	35	24	3	45	1	10	24	40	48		
Mean for Month.	8	41	24	35	42	1	21	24	39	54		

On the 17th and 18th, the wind was violent, accompani with rain, and the needles remarkably unsteady.

Meteorological Observations.

lonth.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Jan.		Inches.				Feet.		
1	Morn....	30·021	35°	94°	NNE		Fine	31½
	Noon....	—	—	—	—		—	39
	Even	—	—	—	—		—	
2	Morn....	30·000	34	79	SE by S		Cloudy	29
	Noon....	30·000	36	63	SE by S		Foggy	38
	Even	—	—	—	—		—	
3	Morn....	29·890	35	70	SSE		Cloudy	33⅔
	Noon....	29·849	39	67	S		Fine	39
	Even	—	—	—	—		—	
4	Morn....	29·755	32	100	SSE		Very fine	31½
	Noon....	29·700	38	74	S		Fine	39
	Even	—	—	—	—		—	
5	Morn....	29·716	34	98	SE		Cloudy	32
	Noon....	29·716	39	80	S		Clouds fer	40
	Even	—	—	—	—		—	
6	Morn....	29·728	33	97	SE		Foggy	32
	Noon....	29 683	40	68	SSE		Cloudy	41
	Even	—	—	—	—		—	
7	Morn....	29·420	38½	87	SSE		Cloudy	35
	Noon....	29·287	42	61	SSE		Cloudy	42
	Even	—	—	—	—		—	
8	Morn....	29·400	35	83	WNW		Clear	34
	Noon....	29·423	39	58	SW by W		Very fine	39½
	Even	—	—	—	—		—	
9	Morn....	29·123	44	66	SSW		Cloudy	36
	Noon....	29·063	44	74	SSW		Stormy	48
	Even	—	—	—	—		—	
10	Morn....	29.436	40	80	SSW		Cloudy	37½
	Noon....	29·339	47	64	SSW		Cloudy	51
	Even	—	—	—	—		—	
11	Morn....	29·233	40	68	SSW		Rain	39
	Noon....	29·304	44	54	W by S		Very fine	44
	Even	—	—	—	—		—	
12	Morn....	29·671	35	72	SW		Very fine	34
	Noon....	29·678	47	58	WSW		Fine	48
	Even	—	—	—	—		—	
13	Morn....	29·524	36	78	S		Very fine	33
	Noon....	29·454	43	67	SW by S		Cloudy	46½
	Even	—	—	—	—		—	
14	Morn....	29·455	46	88	SW		Rain	33
	Noon....	29·400	51	72	WSW		Rain	51
	Even	—	—	—	—		—	
15	Morn....	29·533	39	86	WSW		Cloudy	37½
	Noon....	29·451	47	69	W by S		Sm. rain	48
	Even	—	—	—	—		—	
16	Morn....	29·749	35	58	WNW		Very fine	34
	Noon....	29·772	39	50	W by N		Cloudy	42
	Even ...	—	—	—	—		—	
17	Morn....	29·000	45	91	SW		Rain, stormy	36½
	Noon....	28·871	45	55	W by S		Stormy	48
	Even	—	—	—	—		—	
18	Morn....	28·893	38	58	W		Stormy	36½
	Noon....	29·042	39	55	W by N		Sleet	41
	Even	—	—	—	—		—	

Meteorological Observations continued.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Jan.		Inches.				Feet.		
19	Morn....	29·304	35°	55°	W by N		Fine	33°
	Noon....	29·316	40	49	WNW		Cloudy	41½
	Even....	—	—	—	—		—	
20	Morn....	29·080	34	73	W		Cloudy	33
	Noon....	29·110	39	50	W		Very fine	40¼
	Even....	—	—	—	—		—	
21	Morn....	28·920	34	68	W		Very fine	32
	Noon....	28·977	40	48	W by N		Very fine	41
	Even....	—	—	—	—		—	
22	Morn....	29·044	34	95	SSW		Cloudy	32
	Noon....	28·943	42	80	S by W		Rain	46
	Even....	—	—	—	—		—	
23	Morn....	29·123	33	78	SSW		Very fine	32
	Noon....	29·157	43	54	WSW		Very fine	43
	Even....	—	—	—	—		—	
24	Morn....	29·105	40	94	ESE		Foggy	35
	Noon....	28·980	44	69	SE		Cloudy	44
	Even....	—	—	—	—		—	
25	Morn....	28·971	36	72	SSE		Cloudy	34½
	Noon....	28·759	41	86	S		Rain	44
	Even....	—	—	—	—		—	
26	Morn....	28·946	37	93	S		Cloudy	36
	Noon....	28·954	43	73	SSW		Very fine	44
	Even....	—	—	—	—		—	
27	Morn....	28·990	41	97	ESE		Foggy	37
	Noon....	28 982	46	80	E		Cloudy	47
	Even....	—	—	—	—		—	
28	Morn....	28·990	43	82	SE		Cloudy	39
	Noon....	28·910	46	60	S		Cloudy	47½
	Even....	—	—	—	—		—	
29	Morn....	29·059	36	88	E by S		Fine	35
	Noon....	29·043	44	51	ESE		Fine	47
	Even....	—	—	—	—		—	
30	Morn....	28·895	39	99	E		Rain.	35
	Noon....	28·860	—	96	ENE		Rain	38
	Even....	—	—	—	—		—	
31	Morn....	29·031	36	96	NNW		Cloudy	35
	Noon....	29·032	39	75	NNW		Cloudy	40
	Even....	—	—	—	—		—	

Rain, by the pluviameter, between noon the 1st of Jan. and noon the 1st of Feb. 1·906 inch. The quantity that fell on the roof of my Observatory, during the same period, 2·022 inches. Evaporation between noon the 1st of Jan. and noon the 1st Feb. 1·400 inch.

ARTICLE XIV.

METEOROLOGICAL TABLE.

819.	Wind.	Barometer.			Thermometer.			Hygr. at 9 a. m.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
t Mon.										
an. 19	N W	29·80	29·45	29·625	44	33	38·5	66	19	☽
20	W	29·57	29·28	29·425	41	33	37·0	70	9	
21	W	29·52	29·20	29·360	40	32	36·0	66		
22	S W	29·72	29·20	29·460	49	34	41·5	89	—	
23	S W	29·72	29·60	29·660	45	33	39·0	75		
24	S E	29·60	29·20	29·400	47	36	41·5	90		
25	S E	29·43	29·10	29·265	47	39	43·0	74	26	
26	S E	29·45	29·35	29·400	47	35	41·0	83		●
27	E	29·44	29·35	29·395	49	38	43·5	96		
28	S E	29·52	29·30	29·410	50	31	40·5	85		
29	E	29·52	29·35	29·435	50	30	40·0	97		
30	E	29·40	29·28	29·340	42	33	37·5	88	43	
31	N W	29·62	29·40	29·510	42	25	33·5	78	—	
d Mon.										
Feb. 1	S W	29·60	29·49	29·545	38	28	33·0	85	—	
2	N W	29·73	29·49	29·610	38	18	28·0	90	—	☾
3	S E	29·73	29·42	29·575	41	25	33·0	88	—	
4	N W	29·80	29·47	29·635	43	31	37·0	75	29	
5	E	29·78	29·46	29·620	47	35	41·0	95	12	
6	W	29·61	29·39	29·500	50	36	43·0	85	17	
7	N W	29·65	29·35	29·500	47	33	40·0	68		
8	W	30·00	29·65	29·825	46	34	40·0	72		
9	S W	29·95	29·63	29·790	51	44	47·5	85	37	
10	N W	30·05	29·58	29·815	49	36	42·5	69		○
11	W	30·03	29·79	29·910	51	34	42·5	72		
12	W	29·79	29·50	29·645	48	33	40·5	69	—	
13	N W	29·90	29·62	29·760	47	27	37·0	64	14	
14	N W	30·12	29·90	30·010	47	25	36·0	64		
15	S W	30·08	29·70	29·890	47	34	40·0	65		
16	S	30·08	29·37	29·725	51	42	46·5	69	10	
		30·12	29·10	29·587	51	18	39·31	78	2·16	

he observations in each line of the table apply to a period of twenty-four rs, beginning at 9 A. M. on the day indicated in the first column. A dash otes, that the result is included in the next following observation.

REMARKS.

First Month.—19. *Cirrus* with *Cirrocumulus*, in lines stretching N and S: rain in the night. 20. A very fine day: *Cirri*, p. m. rain and wind in the night. 21. Slight hoar-frost: *Cirrocumulus.* 22. Fair day: rain and wind, evening. 23. Very fine. 24. Fair: strong breeze: cloudy. 25. Rain, a. m. 26. Fair day: large *Cumuli* appeared, passing to *Cumulostratus* with plumose *Cirri* above: at evening there were indications of the *Stratus*. The *Nimbus* has been frequent during the past week: the wind generally moderate in the day, and strong the fore part of the night. 29. Morning rather overcast: day fine, with the lighter modifications ranging (as frequently of late) in lines N and S. About 10, a. m. in going to London, I observed a solar halo of large diameter, imperfect in its superior and inferior part, except a trace at the vertex, but exhibiting, in the points directly N and S of the sun, *two parhelia*, which continued with a faint variable brightness for about 20 minutes. 30. Wet morning: drizzling most part of the day: wind S E, and then N E. 31. Overcast: rained a little, a. m.

Second Month.—1. Hoar frost, with *Cirri* in the sky pointing upwards from a base: drizzling rain at night. 2. *Snow* (for the first time this season) continuing most part of the forenoon from sun-rise: then, brilliant sunshine, and frost at night, with the Thermometer nearly at the minimum of the present winter. 3. Rather misty and overcast, a. m.: wet evening. 4. Cloudy: fair, a. m.: showers, p. m. 5. Misty, drizzling. 6. Very fine, with *Cumuli*, &c. a. m.: in the afternoon, a squall of wind, with a few drops: in the night a gale followed by rain. 7. Very fine. 8. Fair, with *Cirrostratus* in parallel bars here and there, under uniform haze: at night, a lunar halo, very large and colourless. 9. Wet day: stormy night. 10. Early this morning it was very tempestuous; but the day was fine, with *Cumuli* carried by a moderate gale, and *Cirri* scattered like loose hay above: at night, with *Cirrostratus*, a succession of small, ill-formed, but highly coloured halos. 11. Fine, with *Cumulus*, *Cirrostratus*, and winds. 12. Fine morning, then showers (in London attended with hail), and much wind at night. 13. Fine morning: *Cumuli* capped with *Cirrostratus*: *Nimbi*, p. m. with a transient rainbow. 14. Slight hoar frost: fine, with *Cumulostratus*, and a breeze. 15. Fine: the ground was frozen this morning, and *Cirrocumulus* at the same time above. 16. Overcast morning: wet and windy, p. m. and night.

RESULTS.

Winds Westerly, except a week about the New Moon, when they were East and South-East.

Barometer:	Greatest height	30·12 inches.
	Least	29·10
	Mean of the period	29·587
Thermometer:	Greatest height	51°
	Least	18
	Mean of the period	39·31
Mean of the Hygrometer		78
Evaporation		0·65 in.
Rain		2·16 inches.

The observations on the Thermometer, Hygrometer, and Wind, were made at the Laboratory.

Tottenham, *Second Month*, 20, 1819.

L. HOWARD.

ANNALS

OF

PHILOSOPHY.

APRIL, 1819.

Article I.

Researches on the Measure of Temperatures, and on the Laws of the Communication of Heat. By MM. Dulong and Petit.

(*Continued from p.* 182.)

Of Cooling in a Vacuum.

The observations on the cooling in vacuo, calculated as before explained, are all affected by an error very small indeed, but which it is requisite to correct. This error comes from the small quantity of air remaining in the balloon, and which, in the greater number of the experiments, amounted only to two millimetres.

This correction cannot be applied immediately to the series of temperatures furnished by observation; but it can be easily applied to the velocities of cooling obtained by calculation. It is merely necessary to diminish them by a quantity corresponding to the heat carried off by the air remaining in the balloon.

To determine the amount of this correction in each case, we observed the cooling of our thermometer in air of different degrees of density; and we calculated for the different excesses of temperature the velocities of cooling corresponding to each density. By subtracting from these velocities those which take place in vacuo, we obtain exactly the quantities of heat carried off by the air in its different states of rarefaction. We shall have nearly accurate values of these same quantities by subtracting the velocities already very near each other, which are given by the observations of cooling in the balloon, when it contains only a very small quantity of gas.

Having thus determined for each excess of temperature and for different densities the quantities of heat carried off by the air, we observed that they followed a simple law, by means of which we determined with sufficient precision the corrections which the calculated velocities ought to undergo. The numbers, therefore, which we shall give in the subsequent part of this chapter, may be considered as differing exceedingly little from those which would be obtained by making the experiments in an absolute vacuum.

Let us now proceed to the examination of the different series calculated and corrected, and let us begin with that in which the balloon was surrounded by melting ice. The thermometer preserved its natural vitreous surface.

Excess of the therm. above the balloon.	Corresponding velocities of cooling.
240°	10·69°
220	8·81
200	7·40
180	6·10
160	4·89
140	3·88
120	3·02
100	2·30
80	1·74

The first column contains the excesses of temperature of the thermometer above the walls of the balloon; that is to say, the temperatures themselves since the balloon was at 0°. The second column contains the corresponding velocities of cooling, calculated and corrected by the methods already pointed out. These velocities, as we have already observed several times, are the number of degrees that the thermometer would sink in a minute, supposing the cooling uniform during the whole minute.

This first series shows clearly the inaccuracy of the law of Richmann; for, according to that law, the velocity of cooling at 200° should be double that at 100°; whereas we find it more than triple. When we compare in like manner the loss of heat at 240° and at 80°, we find the first about six times greater than the last; while, according to the law of Richmann, it ought to be merely triple.

Nothing would be easier than by a formula composed of two or three terms to represent the results contained in the preceding table, and to obtain in this way an empirical relation between the temperatures of bodies and the corresponding velocities of cooling. But formulas of this kind, though without doubt they are useful when we wish to interpolate, are almost always inaccurate beyond the limits within which the observations have been made, and never contribute to make us acquainted with the laws of the phenomena which we study.

We have thought it necessary, therefore, before endeavouring to find any law, to vary our observations as much as the nature of the subject would admit; and we have been guided in this by a remark relative to the theory of radiation, which, we think, has not hitherto been made by any philosopher.

In the theory of the exchanges of heat which has been adopted, the cooling of a body in vacuo is merely the excess of its radiation above that of the surrounding bodies. Therefore, if we call θ the temperature of the substance surrounding the vacuum in which the body cools, and $t + \theta$ the temperature of the body, we shall have in general for the velocity, V, of cooling (observing that this velocity is null when t is null),

$$V = F(t + \theta) - F(\theta)$$

F denoting the unknown function of the absolute temperature, which represents the law of radiation.

If the functions $F(t + \theta)$ and $F(\theta)$ were proportional to their variables; that is to say, if they were of this form, $m(t + \theta)$ and $m(\theta)$; m being a constant quantity, we should find the velocity of cooling equal to $m\,t$, and we should fall into the law of Richmann; since the velocity of cooling would be proportional to the excesses of temperature. These velocities would be at the same time independent of the absolute temperatures, as has been hitherto supposed. But if the function, F, be not proportional to its variable, as our experiments prove, the expression

$$F(t + \theta) - F(\theta),$$

which represents the velocity of cooling, ought to depend at once upon the excess of temperature t and the absolute temperature θ of the surrounding medium. It was to vary this consequence that we observed the cooling of the thermometer in vacuo, raising successively the water surrounding the balloon to 20°, 40°, 60°, 80°. The following table presents, in the same point of view, all the results of each of these series of observations, which were repeated several times.

Excess of temperature of the thermometer.	Velocity of cooling water at 0°.	Ditto water at 20°.	Ditto water at 40°.	Ditto water at 60°.	Ditto water at 80°.
240°	10·69	12·40°	14·35°	—	—
220	8·81	10·41	11·98	—	—
200	7·40	8·58	10·01	11·64°	13·45°
180	6·10	7·04	8·20	9·55	11·05
160	4·89	5·67	6·61	7·68	8·95
140	3·88	4·57	5·32	6·14	7·19
120	3·02	3·56	4·15	4·84	5·64
100	2·30	2·74	3·16	3·68	4·29
80	1·74	1·99	2·30	2·73	3·18
60	—	1·40	1·62	1·88	2·17

This table, which requires no explanation, confirms, as is evident, the principle which we have established; but the results which it contains lead us to a very simple approximation which discovers the law of cooling in vacuo. If we compare the corresponding numbers of the second and third column; that is to say, the velocities of cooling for the same excess of temperature, the surrounding medium being successively at 0° and at 20°, we find that the ratios of these velocities vary as follows:

1·16 .. 1·18 .. 1·16 .. 1·15 .. 1·16 .. 1·17 .. 1·17 .. 1·18 .. 1·15

These numbers, which differ very little from each other without showing any regularity in their variations, require to be rendered equal changes in the velocities observed, which would scarcely amount to one per cent.

Let us now compare the velocities observed when the surrounding medium was at 20° and 40°. We shall find for the ratios of these velocities:

1·16 .. 1·15 .. 1·16 .. 1·16 .. 1·17 .. 1·16 .. 1·17 .. 1·15 .. 1·16 .. 1·16

When the surrounding medium is at 40° and 60°, the ratios are:

1·15 .. 1·16 .. 1·16 .. 1·15 .. 1·17 .. 1·16 .. 1·18 .. 1·16

When the surrounding medium is at 60° and 80°, the ratios are:

1·15 .. 1·15 .. 1·16 .. 1·17 .. 1·16 .. 1·17 .. 1·17 .. 1·15

These last three comparisons lead us to the same conclusion as the first, and inform us, besides, that the constant ratio between two consecutive series has remained always the same when the surrounding medium was heated from 0° to 20°; from 20° to 40°; from 40° to 60°; and from 60° to 80°. The preceding experiments then prove the following law: *The velocity of cooling of a thermometer in vacuo for a constant excess of temperature, increases in a geometrical progression, when the temperature of the surrounding medium increases in an arithmetical progression. The ratio of this geometrical progression is the same whatever be the excess of temperature considered.*

This first law, which applies solely to the variation of temperature of the surrounding medium, enables us to put the expression found formerly of the velocity of cooling in vacuo,

$$F(t + \theta) - F(\theta)$$

under the form

$$\varphi t + a^{\theta}.$$

a being a constant quantity, and $\varphi(t)$ a function of the variable t only, and which we must endeavour to discover.

The two expressions of the velocity of cooling being equal, we have

$$\frac{F(t + \theta) - F(\theta)}{a^{\theta}} = \varphi(t).$$

Whence by developing the series

$$\varphi(t) = t\,\frac{F'(\theta)}{a^\theta} + \frac{t^2}{2}\cdot\frac{F''(\theta)}{a^\theta} + \frac{t^3}{2\cdot3}\cdot\frac{F'''(\theta)}{a^\theta}.$$

And as this equation must hold good for all values of t, we must have

$$F'\,\theta = n\,.\,a^\theta$$

n being an indeterminate number; whence we deduce:

$$F(\theta) = m\,.\,a^\theta + \text{a constant quantity.}$$

Making, for the sake of shortness, $\frac{n}{\log.\,a} = m$, we get

$$F(t+\theta) = m\,.\,a^{t+\theta} + \text{a constant quantity.}$$

We have then finally for the value of the velocity,

$$V = m\,.\,a^\theta\,(a^t - 1)$$

an equation which contains the law of cooling in vacuo.

If we suppose θ constant, the coefficient $m\ a^\theta$ will be so likewise, and the preceding law may be announced in this manner.

When a body cools in vacuo, surrounded by a medium whose temperature is constant, the velocity of cooling for excess of temperature in arithmetical progression, increases as the terms of a geometrical progression, diminished by a constant quantity.

The ratio a of this progression is easily found for the thermometer, whose cooling we have observed; for when θ augments by 20°, t remaining the same, the velocity of cooling is then multiplied by 1·165, the mean of all the ratios determined above. We have then

$$a = \sqrt[20]{1{\cdot}165} = 1{\cdot}0077$$

It only remains, in order to verify the accuracy of this law, to compare it with the different series contained in the table inserted above. Let us begin with that in which the surrounding medium was 0°. We find in this case that it is necessary to make $m = 2{\cdot}037$; we have then for this case,

$$V = 2{\cdot}037\,(a^t - 1)$$

in which $a = 1{\cdot}0077$.*

Excesses of temp. or values of t.	Values of V observed.	Values of V calculated.
240°	10·69°	10·68°
220	8·81	8·89
200	7·40	7·34
180	6·10	6·03
160	4·89	4·87
140	3·88	3·89
120	3·02	3·05
100	2·30	2·33
80	1·74	1·72

* To recollect the value of the coefficient 1·0077, we may remark, that it is nearly equal to the square of the coefficient of the dilatation of the gases.

Let us now take the series obtained when the surroundin medium was at 20; the preceding coefficient of $(a^t - 1)$ mu then be multiplied by $a^{20} = 1{\cdot}165$. We have then

$$V = 2{\cdot}374\,(a^t - 1)$$

Excesses of temp. or values of t.	Values of V observed.	Values of V calculated.
240°	12·40°	12·46°
220	10·41	10·36
200	8·58	8·56
180	7·04	7·01
160	5·67	5·68
140	4·57	4·54
120	3·56	3·56
100	2·74	2·72
80	1·99	2·00
60	1·40	1·38
40	0·86	0·85
20	0·39	0·39

Let us proceed to the series obtained when the surroundin medium was at 40°. The preceding coefficient of $(a^t - 1)$ mus still be multiplied by $a^{20} = 1{\cdot}165$. Hence

$$V = 2{\cdot}766\,(a^t - 1)$$

Excesses of temp. or values of t.	Values of V observed.	Values of V calculated.
240°	14·35°	14·44°
220	11·98	12·06
200	10·01	9·97
180	8·20	8·17
160	6·61	6·62
140	5·32	5·29
120	4·15	4·14
100	3·16	3·17
80	2·30	2·33
60	1·62	1·61

For the series in which the surrounding medium is 60°, we shall have

$$V = 3{\cdot}222\,(a^t - 1$$

Excesses of temp. or values of t.	Values of V observed.	Values of V calculated.
200°	11·64°	11·61°
180	9·55	9·52
160	7·68	7·71
140	6·14	6·16
120	4·84	4·82
100	3·68	3·69
80	2·73	2·71
60	1·88	1·87

Lastly, when the surrounding medium is 80°, we have

$$V = 3{\cdot}754\,(a^t - 1)$$

Excesses of temp. or values of *t*.	Values of V observed.	Values of V calculated.
200°	13·45°	13·52°
180	11·05	11·09
160	8·95	8·98
140	7·19	7·18
120	5·64	5·61
100	4·19	4·30
80	3·18	3·16
60	2·17	2·18

The remarkable agreement of the results of calculation and observation leaves no doubt of the accuracy of the law to which we have been led. Without stopping at the consequences which may be deduced from it, and upon which we shall return immediately, let us examine the series of the velocity of cooling when the bulb of the thermometer is covered with silver. When these series were calculated, we immediately perceived, on comparing them with the analogous series when the thermometer was naked, that the velocities of cooling in these last, for the same temperature of the surrounding medium and the same excess of temperature of the body, were proportional to the corresponding velocities of cooling when the bulb was silvered. The formula found above, then, will apply also in the case of silver, preserving to a the same value, and diminishing m properly.

Our first observation on the cooling of the silvered thermometer was made, θ being equal to 20°. We found that it was necessary to suppose $m = 0{\cdot}357$, and consequently $m\,a^\theta = 0{\cdot}416$. Hence

$$V = 0{\cdot}416\,(a^t - 1)$$

Excesses of temp. or values of *t*.	Values of V observed.	Values of V calculated.
280°	3·05°	3·11°
260	2·59	2·61
240	2·18	2·18
220	1·83	1·81
200	1·53	1·50
180	1·26	1·23
160	1·02	1·00
140	0·81	0·80
120	0·62	0·62
100	0·47	0·48
80	0·34	0·35
60	0·24	0·24
40	0·15	0·15
20	0·07	0·07

A series so extensive as the preceding is sufficient to prove that the formula which applies to the cooling of the glass bulb in vacuo extends likewise to the case of the silver bulb, preserving to. a the same value. However, not to neglect any of the means of verification in our power, we altered the temperature of the surrounding medium, and raised it to 80°. The preceding coefficient of $(a^t - 1)$ must be multiplied by a^{60}; which gives

$$V = 0{\cdot}658\,(a^t - 1)$$

Excesses of temp. or values of t.	Values of V observed.	Values of V calculated.
240°	3·40°	3·44°
220	2·87	2·86
200	2·35	2·37
180	1·92	1·94
160	1·56	1·58
140	1·27	1·26
120	0·99	0·98
100	0·75	0·76
80	0·56	0·55

The simplicity and generality of the law which we have just established, the precision with which it is confirmed by observation through an extent of nearly 300° of the thermometric scale, show clearly that it will represent rigidly the rate of cooling in vacuo at all temperatures and for all bodies.

Let us now return to the calculation which led us to the discovery of this law.

The total radiation of the surrounding medium is represented in it by F (θ), and we find for its value,

$$m\, a^{\theta} + \text{a constant quantity.}$$

But the point of commencement from which the absolute temperatures θ are reckoned being arbitrary, we may choose it in such a way that the constant quantity shall be null; which will reduce the expression to $m\, a^{\theta}$. We may conclude then that if it were possible to observe the absolute cooling of a body in a vacuum; that is to say, the loss of heat by the body, without any restoration on the part of the surrounding bodies, this cooling would follow a law in which the velocities would increase in a geometrical progression, while the temperatures increase in an arithmetical progression; and further, that the ratio of this geometrical progression would be the same for all bodies, whatever the state of their surfaces may be.

From this law, very simple in itself, is deduced as a consequence, that of the real cooling of bodies in vacuo; a law which we have already announced above. In fact, to pass from the first case to this, it is only necessary to take into the account the quantity of heat sent back every instant by the surrounding medium. This quantity will be constant, if the temperature of the surrounding medium does not vary. Hence it follows, that the real velocity of cooling of a body in vacuo ought, for excesses of temperature in an arithmetical progression, to increase in a geometrical progression, diminished by a constant quantity. This number itself must vary itself, according to a geometrical progression, when the temperature of the surrounding medium (of which it represents the absolute radiation) varies according to an arithmetical progression. These different results are clearly expressed in the equation obtained above, making $m\,a^{\theta} = \mathrm{M}$. We have

$$\mathrm{V} = \mathrm{M}(a^{t} - 1)$$

M is the number which we must take from the different terms of the geometrical progression expressed by M a^{t}; and we see, besides, that this number M is connected with θ by the relation announced above.

Since the value of a is independent of the nature of the surface, it follows that the law of cooling in vacuo is the same for all bodies; so that the radiating power of different substances preserves the same ratio at all temperatures. We have found this ratio equal to 5·7 on comparing glass with silver. This result is a little less than that of Mr. Leslie; owing, no doubt, to the surface of our silvered thermometer being tarnished, while that of Mr. Leslie's was polished. We see likewise, according to the hypothesis which has given us the law of absolute radiation, that we must make $\theta = \infty$ to render the velocity null; which fixes the absolute zero at infinity. This opinion, rejected by a great many philosophers, because it leads to the notion that the quantity of heat in bodies is infinite, supposing their capacity constant, becomes probable, now that we know that the specific heats diminish as the temperatures sink. In fact, the law of this diminution may be such that the integral of the quantities of heat, taken to a temperature infinitely low, may, notwithstanding, have a finite value.

The law of cooling, such as we have represented it, and such as it may be represented in vacuo, applies solely to the velocities of cooling, estimated by the diminutions of temperature indicated by an air thermometer. We may see by the correspondence of all the thermometrical scales given in the first part of this memoir, that if we make use of any other thermometer, the relations between the temperatures and velocities of cooling would lose that character of simplicity and generality which we have found them to possess, and which is the usual attribute of the laws of nature. If the capacities of bodies for heat were constant when we determine them by the same thermometer, the preceding law would still give the expression of the quantities of heat lost, in a function of the corresponding temperatures. But as we have proved that the specific heat of bodies is not constant in any thermometrical scale, we see that, in order to arrive at these real losses of heat, we must admit an additional element; namely, the variation of the capacity of the bodies subjected to observation. In considering the question under this point of view, it would be necessary to know, in the first place, the law of the capacities for a certain body, and to determine then, by direct observations, the quantities of heat lost by the same body at certain fixed terms of temperature indicated by the air thermometer. Then by multiplying the velocities of cooling deduced from the preceding law by the corresponding capacities, we should obtain the absolute losses of heat. It is not in the interval of the first two or three hundred degrees of the centigrade scale that we can hope to verify the accuracy of these consequences. The variation of the capacities not beginning to become very sensible till we pass that term, it would be necessary to observe at temperatures of 5 or 600°. It is easy to see the difficulty of such a kind of observation. However, we have succeeded in constructing apparatus fit for the purpose; and we have already made a great many observations relative to this subject. But as our results do not yet present all the regularity which we expect to be able to give them, we have determined to delay their publication; and so much the more willingly, that the question which it is their object to answer does not come within the limits of the prize proposed by the Academy.

The method which Mr. Leslie employed for measuring the radiating powers of different surfaces is very good for making us acquainted with the radiating heat lost by a body at all temperatures. It is well known that his method consists in estimating the radiation of a body by the heat communicated to an air or mercurial thermometer placed at a certain distance from the hot body; and to render the effects more sensible, this thermometer is placed at the focus of the reflector.

It was by means of this apparatus that Laroche obtained the result which we have mentioned above. Among the series of observations made by this method, there is one which extends indeed to very high temperatures; but it cannot be of any utility, because the temperatures were determined by a process founded on the supposition that the capacities were constant. The numbers which represent the losses of heat are besides affected by another error, proceeding from the heat of the focal thermometer being too great, because the inaccuracy of the Newtonian law had already become very sensible. But to show that our law satisfied the observations made by this process when they are freed from the causes of error of which we have just spoken, we shall apply them to the series given in the same memoir which do not go beyond the limits in which the variation of capacity produces no sensible effect. These series are the radiation of an iron crucible filled with mercury. Here the temperature of the body not having exceeded 200°, we may suppose the specific heat constant. We may likewise neglect the correction which the mercurial thermometer would require to bring it to the air thermometer; because this correction is very small, and because it is probable that it is more than compensated, from the stem of the thermometer employed by Laroche not being completely plunged into the mercury.

Instead of taking each of the series which is given by this philosopher, we have taken in some measure the mean of them, assisted by the formula by which M. Biot has represented these observations—a formula inserted in page 634 of the fourth volume of his Traité de Physique. The numbers which we give as the result of observation are then deduced from the formula of M. Biot. To represent them by means of our law, we must make V, which here represents the radiation, equal to

$$4{\cdot}24\,(a^t - 1)$$

t being the excess of the temperature of the crucible, and a a constant quantity, which we have found precisely equal to 1·0077.

Values of t.	Values of V observed.	Values of V calculated.
200°	15·33°	15·29*
180	12·51	12·52
160	10·09	10·15
140	8·04	8·11
120	6·30	6·36
100	4·84	4·86
80	3·60	3·58
60	2·54	2·47

This last series furnishes by its agreement with our law, a new proof that the number a depends neither upon the mass nor on the state of the surface of the body. Since we find it here to have the same value as in our experiments on cooling vitreous and silvered surfaces in vacuo.

From the expression of the velocity of cooling in vacuo, we can easily deduce the relation which connects the temperatures and the times. If we denote the time by x, we have

$$V = -\frac{dt}{dx} = M\,(a^t - 1)$$

M being a constant coefficient which depends solely on the temperature of the surrounding medium. From this we conclude

$$dx = \frac{-dt}{M\,(a^t - 1)}$$

and

$$x = \frac{1}{M\log. a}\left(\log. \frac{a^t - 1}{a^t}\right) + \text{a constant quantity.}$$

The arbitrary constant quantity and the number M will be determined in each particular case, when we have observed the values of t corresponding to two known values of the time x.

If we supposed t so small that, considering the smallness of the logarithm of a, we could confine ourselves to the terms of the first power in the development of a^t, we should obtain the Newtonian law.

(*To be continued.*)

ARTICLE II.

On the Hypothesis of Mr. Knight, accounting for the Direction of the Radicle and Germen. By the Rev. Patrick Keith.

(To Dr. Thomson.)

SIR, *Betherden Vicarage, Kent, Jan. 29, 1819.*

IN your number for April, 1817, you have given an account of the proceedings of the Linnæan Society of London, by which it appears that, on Tuesday, March 4, preceding, a paper, by T. A. Knight, Esq. was read to the Society, containing a defence of what is called my attack upon his hypothesis, of which I unfortunately exhibited an inaccurate representation. This inaccuracy I have since admitted and apologized for in the proper quarter, but not to the prejudice of any future inquiry. If Mr. Knight's hypothesis is founded in truth, it will suffer nothing from my investigation; and if it is founded in error, the sooner the error is detected the better. On this account, I have given it a second perusal, and I am desirous of communicating the result of it to the public through the medium of your *Annals.* But that I may not again exhibit an incorrect view of Mr. Knight's hypothesis, I will give it in his own words, as it occurs in an inference drawn from the two following experiments.

Exper. I.—On the circumference of a vertical wheel performing 150 revolutions in a minute, by which the influence of gravitation was conceived to be wholly suspended, beans were placed in all directions. The result was, that the radicles uniformly turned their points outwards from the circumference of the wheel; and in their subsequent growth receded nearly at right angles from its axis. The germens, on the contrary, took the opposite direction; and in a few days their points all met in the centre of the wheel. They even extended beyond it; but the same cause which first occasioned them to approach its axis still operating, their points returned, and met again at the centre.*

Exper. II.—In consequence of some slight objections which Mr. Knight anticipated as likely to be alleged against the conclusion he was inclined to draw from the foregoing experiment, a second experiment was instituted, by adding to the former machinery a horizontal wheel, which was made to perform 250 revolutions in a minute, and to the circumference of which, beans were fastened as before. The issue was, that the radicles were protruded outwards and downwards, about 10° below, and the germens inwards and upwards, about 10° above the plane of the wheel. But when the rapidity of the wheel's motion was diminished, the radicles were more perpendicular, and the germens more upright; 80 revolutions in the minute giving an elevation and depression to the stem and root respectively of 45°.*

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* Phil. Trans. 1806. Part I. p. 100, 101.

From the foregoing experiments, Mr. Knight infers, "that the radicles of germinating seeds are made to *descend*, and their germens to *ascend*, by some external cause, and not by any power inherent in vegetable life; and that there is but little reason to doubt that gravitation is the principal, if not the only agent employed in this case by nature." †

With regard to the first experiment, it may be remarked, that the anticipated objection is not quite so slight as Mr. Knight seems to have imagined; for as the radicles were, at least during the one-half of their circumvolution, in their natural position, or nearly so, while the artificial centrifugal force was operating rather in conjunction with gravitation, or in the direction in which radicles naturally grow, so as to do more than counterbalance its effect in the other half of the circumvolution, in which the force of gravitation was opposed to it, it may be said, that there is no new case put from which any inference can be drawn; and the moment the stems passed the centre, it was to them the same thing as growing downwards, which it is known that they cannot do. But the experiment seems to me to be liable to a much more serious objection than that which Mr. Knight had anticipated; for, as in this case the influence of gravitation was conceived to be wholly suspended, and the radicles subjected to the agency of the centrifugal force alone, they ought surely to have been protruded in the direction of that force. Now the direction of the centrifugal force in question must of necessity have been oblique, as being the simple effect of circular motion; and not the reverse of that of gravitation, like an arrow shot from a bow perpendicularly upwards. Why then were the radicles protruded at right angles to the axis of the wheel? If one of the beans had by any accident lost its hold, would it have been thrown off from the circumference of the wheel in that direction? Unquestionably not. It would have been thrown off in the direction of a tangent to the orbit which it was describing; and in this direction also the radicle ought to have been elongated, the direction of the plumelet being the reverse.

The second experiment is thought to be the most decisive, and we may fairly allow it to be the most plausible of the two; though the account that is given of it by Mr. Knight leaves a *desideratum* that greatly diminishes its importance. We are told that the radicles were protruded outwards and downwards at about 10° below, and the germens inwards and upwards at about 10° above the plane of the wheel's orbit; but we are n told whether this approach or recession was in the plane of t wheel's axis, or otherwise; and if otherwise, then we are not told any thing with respect to the degree of its deviation, or how it was affected by the increased or diminished velocity of the wheel; all which seems to be absolutely indispensable to Mr. Knight's conclusion; for if the velocity had been such as to counteract the force of gravitation completely, then, upon Mr. Knight's principles, it is evident, that the radicle ought to have been protruded, not merely outwards and downwards, but horizontally; and not yet merely horizontally, but in the direction of a tangent to the orbit of the bean, like the drops of water that flew off from the rim of Mr. Knight's main wheel; or (to take a more familiar example), like the drops of water that fly off from the tags of a twirled mop. It ought, therefore, to have been making approaches to this direction according to the degree of velocity with which the wheel's motion was accelerated. Will it be said that the resistance of the air prevented it from approaching, or from assuming that direction? Then the resistance of the air ought, for the very same reason, to have acted upon the radicles of the beans that were fixed to the circumference of the vertical wheel, and to have affected their direction also. But of this we find not the slightest intimation; and if it had even done so, there is no reason to believe that its action would have stopped just at right angles to the axis of the wheel. Hence it is evident that Mr. Knight's conclusion does not legitimately follow from the premises which his experiments present.

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* Phil. Trans. 1806. Part I. p. 102, 103.

† Ibid. 103.

We do not, however, deny that gravitation, or a power counteracting gravitation, may affect the growth of plants, and influence the direction of the root or stem; or that the effect produced by a foreign force will be in proportion to the amount of the force impressed; but we contend that the vegetating plant possesses energies capable of counteracting the influence of gravitation when necessary; and that gravitation is not the sole, nor even the principal agent employed by nature to give direction to vegetables. It is indeed a grand trial of our faith to have to believe that the roots of plants grow downwards and the stems upwards merely by the agency of gravitation. But if it were even granted, still the phenomenon would remain an incomprehensible paradox till duly explained, notwithstanding the result of the two experiments; and accordingly Mr. Knight endeavours to point out the means by which gravitation may produce the diametrically opposite effects which his hypothesis ascribes to it.

He begins by saying, that "the radicle of a germinating seed (as many naturalists have observed) is increased in length only by new parts successively added to the apex, or point, *and not at all by any general extension of parts already formed;* so that the matter added being fluid, or changing from a fluid to a solid state, may be supposed to be sufficiently susceptible to the influence of gravitation to give an inclination downward to the point of the radicle."

Whether Mr. Knight takes this supposed fact entirely upon the credit of others, or whether he confirmed it by his own observation, I cannot positively decide; though the parenthesis contained in the above quotation renders the former part of the alternative the most probable. There is no doubt that many naturalists have been of this opinion, particularly Du Hamel, who gives a minute account of the experiment by which he seemed to have ascertained the fact. Having passed several threads through the root of a plant, and noted the distances, he then immersed the root in water. The upper threads retained always their relative and original situation, and the lowest thread, which was placed within a few lines of the end, was the only one that was carried down. Hence he concluded that the root is elongated merely by the extremity.*

Resting upon this high authority, I confess that I did till lately assume the fact without examination. But the result of the following experiment will show that the opinion is still incorrect, in spite of all the authorities by which it has been backed.

On Oct. 1, 1818, I sowed some tick beans in a small earthen pan filled with garden mould.

On the 4th, the radicle of the most forward had protruded about $\frac{1}{5}$th of an inch beyond the integuments, when I marked it with ink at the point, in the middle, and at the base, as clearing the integuments; so that the marks were about $\frac{1}{10}$th of an inch from each other.

On the 5th, the radicle was $\frac{1}{4}$th of an inch in length, and the marks nearly as before with regard to their relative distances, but removed evidently from the integuments, so as to admit of a fourth or additional mark again adjoining the integuments. The radicle, which was originally upright, was now bending down.

On the 6th, the radicle was $\frac{1}{2}$ an inch in length, the first mark being within two or three lines of the point; the second at about $\frac{1}{8}$th of an inch from the first; the third at about $\frac{1}{6}$th of an inch from the second; and the fourth at about $\frac{1}{8}$th of an inch from the third; as well as perceptibly removed from the integuments.

On the 7th, the radicle was $\frac{3}{4}$th of an inch in length. The first mark was still within two or three lines of the point; the second was at the distance of $\frac{1}{4}$ of an inch from the first; the third was at the distance of $\frac{1}{4}$ of an inch from the second; and the fourth was at about the distance of $\frac{1}{8}$th of an inch from the third, being but little more than its original distance, but removed to the distance of $\frac{1}{8}$th of an inch from the integuments.

On the 8th, the radicle was one inch in length, the first mark being still near the apex; the second at the distance of about

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* Phys. des Arb. lib. i. chap. v.

⅓d of an inch from the first; the third at the distance of about ⅓d of an inch from the second; and the fourth nearly as before.

On the 9th, the radicle was 1¼ inch in length, the three marks next the base being nearly as before, and the mark next the apex being the only one that was carried down.

On the 10th, and as long as any further observations were made, it was still the lower extremity of the radicle, and that only, which was carried down. But enough had been previously observed to show that the assumed peculiarity of the elongation of the radicle is founded in a mistake; and that the root in its incipient state, like the stem in *its* incipient state, is augmented by the introsusception and deposition of additional particles throughout its whole mass; or "by a general extension of parts already formed;" though it may afterwards, like the stem, become so firm and compact as no longer to admit of augmentation in that way. I suspect, therefore, that Du Hamel's experiment was not instituted at a sufficiently early period of the radicle's or root's growth; or that it was somehow or other unnaturally affected by being placed in water; or that there are exceptions to the rule, which my experiment establishes.

The bean, which was the subject of the above experiment, grew, as has already been stated, in garden mould, and was taken up and planted again at every observation. My observations were not, however, confined to that single bean; they were extended to many others, as well as to the radicles of mustard, cress, and radish seed, all which gave similar results; so that if there are any exceptions to the rule which my experiment establishes, the radicle of the bean, on which Mr. Knight's two experiments were made, is not one of them.

Thus it is proved that the facility with which the germinating radicle might be influenced by the agency of gravitation from the supposed peculiarity of its mode of growth is wholly imaginary; and if it were even the fact, still the particles by which it is augmented, though originally fluid, or changing from the fluid to the solid state, are contained within an epidermis which bounds and confines them, and are not committed to the influence of gravitation merely, like the trickling drops of water that are added to the point or surface of an icicle.

If any other evidence were wanted to prove the fact that the root is augmented by the introsusception and deposition of additional particles throughout its whole extent, I would adduce the case of the garden radish when past the stage of germination. In taking up young radishes that are just fit for the table, it is no uncommon thing to meet with an individual that is elevated at the collar by at least an inch above the surface of the soil. But how is this elevation to be accounted for except upon the principle now assumed? It must not be said that the base of the root has been pushed upwards, because the apex could not get downwards; for the apex has been descending all the while, and will continue indeed to descend to a much greater depth, if not prematurely taken up. At this moment there lies before me the root of a radish sown in the month of March or April last, and which from being allowed to stand to come to seed, and not taken up till a few days ago, measures one foot in length from the base to the apex, with a diameter of 2½ inches at the widest, three inches having been raised above the soil, and nine buried under it. Now it is evident that this growth was occasioned by the introsusception and deposition of particles throughout its whole extent. The turnip seems also to be an example in point.

Warranted, therefore, by these facts, I contend that gravitation finds no facility whatever in carrying down the radicle which it would not find also in carrying down the plumelet; and that whatever may be its agency upon the one, it ought to be precisely the same upon the other. If the root grows downwards by virtue of gravitation, so should the stem; and hence by force of the counteracting power in Mr. Knight's experiment, both root and stem ought to have receded from the circumference of the wheels outwards, like a thong of leather nailed to it by the middle, and the machinery put in motion. Such is Mr. Knight's account of the *rationale* of the descent of the root. Let us now proceed to his account of the *rationale* of the ascent of the stem.

" If (says Mr. Knight) the motion and consequent distribution of the true sap be influenced by gravitation, it follows that when the germen, at its first emission, or subsequently, deviates from a perpendicular direction, the sap must accumulate on its under side."* But the motion and consequent distribution of the true sap is proved to be very little, if at all, influenced by gravitation, from the fact of its easy ascent in the pendant shoot of the weeping willow, and in other pendant shoots—a fact that Mr. Knight will not refuse to acknowledge; so that the principle on which his argument rests is altogether gratuitous. He regards it, however, as resting upon facts; for he further says, " I have found in a great variety of experiments on the seeds of the horse-chesnut, bean, and other plants, when vegetating at rest, that the vessels and fibres on the under side of the germen invariably elongated much more rapidly than those on its upper side, and thence it follows that the point of the germen must always turn upwards." Nor is this increased elongation confined to the under side of the germens, nor even to the annual shoots of trees; but it occurs and produces the most extensive effects in the subsequent growth of their trunks and branches."

It is to be regretted that no particular account of these experiments is given, nor of the way in which the elongation was ascertained. But it is certain that this elongation does not always take place where a bend [illegible], so as to make the point turn upwards; and that when a pendant stem becomes elevated, the elevation does not commence at the point. In support of this assertion, I will adduce the fact of the spiral growth of the tendril of the vine. If it followed the rule of elongation implied in Mr. Knight's hypothesis, it could not complete so much as a single circumvolution round its supporter (especially if its position should be horizontal), and yet it completes several circumvolutions before it is satisfied with its hold. Further, if plants followed the rule implied in the hypothesis, how should the plumelet of the onion ascend in the shape of a loop, or whip and pendant lash; or how should beans planted at the depth of a foot send up a perpendicular stem with the summits bent down in the shape of hooks till they reach the surface; or how should the frond of *Pteris aquilina* come up and continue so long circinal; or how should the poppy or crown imperial be at all able to rear their nodding heads? But if we allow the alleged elongation to take place, will it give verticality to the plant? I think it will not. For as the operating cause is capable of turning up the point only, the bend will remain as before; and when a new bend takes place, whether on the same side or on the side opposite, it will be the point only that will again ascend; so that the stem will exhibit throughout its whole extent only a succession of bends and turn-ups.

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* Phil. Trans. 1806 [illegible] I. p. 104.

The fact, however, is, that when the pendant stem is elevated, the elevation does not commence at the summit, but at the lower part of the bend; so that this is a case for which Mr. Knight's hypothesis furnishes no provision.

On Sept. 1, 1816, at eight o'clock, a. m. the shoot of a plum-tree was much bent down; the origin of the bend being at least six inches from the summit. At six o'clock, p. m. it had begun to resume an erect position by the lower half, though in a sort of zigzag, or rather waving line.

On the 2d, at 11 o'clock, a. m. the process of erection was still going on in the same waving line, and the summit slightly bent by about two inches.

On the 4th, the waving line was a little higher, and the summit bent only by an inch.

On the 6th, the waving line was quite obliterated, and the shoot vertical; but the summit was the last part that was turned up.*

Also, on April 18, 1818, at six o'clock in the morning, the scapes of a batch of daffodils in front of my study were bent down to the earth, by means of a sharp frost, so that the blossoms were recumbent upon the grass. At nine o'clock they were all erect; but the blossoms were all nodding as before. Consequently they were not elevated by any recurvature at the summit, nor by any elongation of the fibres of the under side, as the effect of advancing vegetation, for they were already at their full growth.

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* *Annals of Philosophy*, No. xlvii.

Indeed so far is the bend from being likely to occasion an increased flow of sap on the under side, that it seems to me to be the most likely means of retarding it; as the vessels on the under side must be too strongly compressed to afford a ready passage for the sap, while those on the upper side occasion no obstruction to it; and it does not in fact appear that there is any accumulation of matter deposited on the under side of horizontal or deflected branches; on the contrary, in examining a number of branches so circumstanced, particularly branches of the ash-tree, I have uniformly found the greatest thickness of woody layers to be on the upper side.

Such are the obstacles that present themselves to Mr. Knight's explanation of the phenomenon in question. But even allowing it to be the true one, what is it that gives occasion to the bend downwards? For it appears that gravitation cannot act till a bend in the stem takes place. Say that the bend is occasioned by accident, or by the natural tendency of heavy bodies to gravitate, or by the stems being so weak and limber that it cannot support itself, or by the plumelet's being deflected in the seed. The conditions required are given; and the cause assigned by Mr. Knight will produce the effect ascribed to it, if possible. But if I take a seed whose plumelet is not deflected, and plant it so as that the plumelet shall point perpendicularly downwards, why should it again turn up? Will it be said that it is made to turn up by means of the great quantity of liquid that is directed into it in the process of germination, as it is said that the pendant stem may be afterwards made to turn up? Then I reply that the great quantity of liquid ought, *à fortiori*, to compel it to point downwards still, since it is acting with its full force in the very direction of gravitation; or, at the least, that if gravitation elevates the bent plumelet, or the plumelet of whatever shape, it ought, for the same reason, to elevate the bent radicle; for I have proved that it finds them both in precisely the same circumstances; so that if Mr. Knight still persists in regarding gravitation as the cause that gives direction to the plant, he will be compelled to look out for a new explanation of the way in which it acts.

Mr. Knight now proceeds to answer objections. Du Hamel had said that gravitation could have but little influence in the direction of the plumelet, were it in the first instance protruded, or afterwards made to grow perpendicularly downwards. This is a case that Mr. Knight seems to regard as apparently hostile to his hypothesis; and it is the case that I have just put. But what is his answer to the objection? Merely that having made many experiments on the inverted seeds of the horse-chesnut and the bean, he found that after a certain time the extremity the radicle began to point downwards, and the extremity of the germen to point upwards.*

Another objection arises from the fact that few branches grow perpendicularly upwards; and that roots always spread horizontally. To this, Mr. Knight replies, that the luxuriant shoots of trees that abound in sap do almost uniformly turn upwards, though the more feeble and slender shoots of the same trees grow in almost every direction, probably because their fibres being more dry, and their vessels less amply supplied with sap, they are less affected by gravitation.† If Mr. Knight's hypothesis is insufficient to account for the direction of the stem, it will be also insufficient to account for the direction of the branches. But it may be observed, that a great flow of sap is not at all necessary to uprightness of growth.

On June 1, 1818, a shoot of a raspberry, of about 20 inches in length, was found to have been broken across, near the base, by a violent gust of wind, and separated entirely from the root, except by a small portion of bark, of about a quarter of an inch in breadth. It was lying flat upon the ground by the whole of its length, but was beginning to ascend by the summit.

On the 8th, the vertical portion at the summit was three inches in length.

On the 15th, the vertical portion at the summit was six inches in length. Thus, there was a rapid and upright increase of the shoot, and yet there could not possibly have been a great flow of sap.

Mr. Knight regards the numerous lateral roots that issue from the primary radicle as assuming a horizontal direction, because they are much less succulent than the primary radicle, and consequently less obedient to gravitation; and because they meet with less resistance from the superficial soil than from the soil below; so that the first and perpendicular root, having executed its office of securing moisture to the plant whilst young, is thus deprived of its proper nutriment, and ceases almost wholly to grow.‡

To this it may be replied, that when the seed is made to germinate in the open air, the lateral shoots issuing from the primary radicle are still horizontal, or nearly so, though the primary radicle itself is not always perpendicular. Of three radicles protruded from three beans germinating at rest, in the dark, and in the open air, by being tied to a small slip of wood placed across an earthen pan at the distance of three inches from the bottom, the first receded gradually from the perpendicular till at about the length of two inches it formed an angle of 40°. The second, at the same length, formed an angle of 45°; and the third, at about the length of one inch, was horizontal by the lower half. What more could have been expected from a horizontal and revolving wheel? I may also add, that the lateral and horizontal shoots, issuing whether from the radicles of herbs, or even of trees and other woody plants, as well as the extreme fibres of the grand divisions of the root itself, are all as perfectly succulent as the most tender radicle, and as completely within the influence of gravitation. Indeed the germinating radicle is not always either very soft or very succulent. The radicle of the bean is a firm and compact substance, even at the time that it may be bending downwards. Let any one make the proper observations by watching the germination of a bean; or by taking up the extreme fibres of the root of a tree, the elm tree, for example, at the distance of 10 or 12 feet from the trunk, and he will find that what I have now asserted, whether with regard to the radicle or to the lateral fibre, is the fact.

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* Phil. Trans. 1806. Part I. p. 106. † Ibid. ‡ Ibid. 107.

Gravitation, therefore, if it were the sole, or even the main cause giving direction to roots, ought still to operate on the lateral fibres with its full effect. The resistance of the subsoil does not, in fact, present any very great difficulty to downward growth; for it is penetrated by many roots that seem to be much less fitted for the operation than others that never approach it. Why does the root of *Triticum repens* creep along horizontally at the depth of two or three inches below the surface, though it is, perhaps, the best fitted of all roots for perpendicular descent, by its being furnished with a fine and stiff point that will often penetrate in a horizontal direction, through substances that are much more firm and compact than the soil in which it grows. Thus it has been known to make its way even through a potato, or Jerusalem artichoke, though it will not descend to any considerable depth; and yet if you sow carrots or parsnips in the same soil, their roots will descend to the depth of a foot or more. It is evidently the effect of an election in the plant.

With regard to the tap root of the oak, of which every body talks, I can say nothing from my own observation. Du Hamel asserts its existence,* and Mr. Knight denies it; and from the number of trees which Mr. Knight examined, he certainly has a right to speak with some confidence; though woodmen who have grubbed up many oak trees, uniformly affirm that they are often furnished with a tap root, extending, in most cases, to the depth of three or four feet, and thick in proportion to the trunk. But, however it may be with the oak tree, there are undoubtedly many plants of which the first and perpendicular radicle or root still continues to grow, and to be of the utmost importance to the individual, as is evident from the examples of the roots of the carrot, the parsnip, and the radish, of which the matured radicle constitutes the main bulk.

In my paper, upon the developme[illegible] seminal germ, published in the Transactions of the Lin[illegible] I stated, as an objection to Mr. Knight's hypothesis, the fact of the upward growth of the radicle of the misseltoe, at least when the seed is lodged on the under side of the supporting branch. But I now find that Mr. Knight obviates the objection by saying, that the misseltoe has no root, and that the part in question gains the bark only by receding from the light, like the stem and tendrils of other parasitical plants.

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* Phys. des Arb. liv. i. chap. v.

I am not acquainted with many plants that are strictly parasitical; but I do not find in those with which I am acquainted any peculiar disposition to recede from the light. The dodder, *Cuscuta europea*, cannot be said to have it, because it twines round a supporting stem from right to left; so that in its very outset, it must rather approach the light than recede from it; and again, in every new spire or gyration, broom-rape, *Orobanche major*, does not fly the light, for it comes up quite erect: and I have seen many plants of the misseltoe, *Viscum album*, whose growth is wholly to the south of the point at which they issue from the stem, as well as chiefly ascending. Hence if any part of the germ of a parasitical seed is found to recede from the light, it is most likely, because it is of the nature of a radicle, since radicles are known to do so. Besides, the embryo of the seed of the missletoe is just like many other embryos, furnished with cotyledons, enclosing a plumelet, and what we are bound to call a radicle (though perhaps caulescent), unless for some good reason with which I am not yet acquainted; because it is that part of the embryo which first begins to shoot in the process of germination, and in a direction opposite to the plumelet.

In this opinion I am supported by an authority which I am sure Mr. Knight will respect, namely, that of the great and illustrious Gœrtner, who expressly describes the embryo of the misseltoe as being furnished with a somewhat swollen and capitate radicle, that is, separated from the cotyledons by a slender stipe. All indeed that is situated beneath the cotyledons, may, in the opinion of Gœrtner, be regarded as a radicle in every embryo whatever;* whereas, with regard to the misseltoe, Mr. Knight's opinion implies that all below the cotyledons is a stem. But will Mr. Knight allow me to cut off the point of it to see whether it will insinuate itself into the bark then? If it is wholly a stem, it ought still to do so. But if it refuses, then it is plain that there was something in the point more than a mere stem. This experiment must be made and succeed before Mr. Knight can establish his position; that is, a graft of the misseltoe must succeed by being bound to the outside of the bark of some stock. If it be said that it would be unfair to cut off the point because it may contain something fitted to make it unite with the supporter, then I contend that this something is the very radicle in question.

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* De Fruct. et Sem. Introd.

It is true that some botanists have regarded parasitical plants as being altogether destitute of roots, applying to them the term *arrhiza*, and, perhaps, Linnæus may be squeezed into the number; because in his distribution of the parts of the plant, he describes only a parasitical stem, and says nothing of a parasitical root;* though Linnæus's authority will not, perhaps, be regarded as of much weight in this case, when it is recollected that he elsewhere † represents the stems of all trees and shrubs as being merely roots above ground. But the most scientific definitions or descriptions of the root, amongst which I include those of Malpighi and Du Hamel, as well as that of our worthy and learned President, Sir J. E. Smith,‡ do evidently include parasitical plants; because they represent that part of the plant by which it attaches itself to the substance on which it grows or feeds, as being the root. Besides, there are some parasitical plants that have even conspicuous roots, as any one who has ever seen a mature and complete plant of *Orobanche major* will acknowledge; and although systematic botanists do describe some plants of the class *cryptogamia* as being destitute of roots, because they have no visible or conspicuous root appearing as a distinct organ, yet the phytologist knows that this is not absolutely correct.

We may regard the embryo of the misseltoe, therefore, as being furnished with a radicle, though not very conspicuous; and it need not be thought strange if it grows occasionally upwards. We find that roots in general possess a capacity of accommodating themselves to circumstances in the direction which they affect, independent of, and even in opposition to, gravitation. The roots of trees, which are planted in a bottom, near to sloping banks, will extend not merely in a horizontal direction, but will follow the direction of the ascent. An ash tree which is so situated, and is now within my view, has roots at the distance of five yards from the trunk elevated at least three feet above the level of the collar. If a piece of the root of the horse-radish, *Cochlearia armoracia*, is planted at the depth of 15 inches, it will send up root shoots erect to the surface of the soil;§ and if it is planted at the surface of the soil, it will no doubt send down root shoots to the same, or to a greater depth. There are even some stems, or at least fronds, that seem to be wholly indifferent to the direction in which they grow. Many of the lichens which grow upwards when situated on the upper side of a branch, are very well content to grow downwards when situated on the under side; or to grow horizontally when attached to the surface of an upright trunk. The *lichen prunastri* may be quoted as an example. Further, if gravitation were the sole cause giving direction to the root, there would be no such thing as a root's selecting the bes vhich roots are well known to do. For then it would have no choice but to descend, unless prevented by an obstacle that could not be surmounted; which might stop it or turn it to the one side, but could not surely make it grow upwards, or ascend a bank; for that would be like making a river to run up a hill.

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* Phil. Bot. sect. 82.

† Ibid. se

‡ Introd. to Bot. 102.

§ Mawe's 's Dictio

In short, the more we examine the subject, the more we feel the want of a principle "inherent in vegetable life" to determine the direction of the plant. We see that such a principle must be the cause of many of the other phenomena of vegetation, and why not also of the phenomenon in question. To what but to the operation of such a principle are we to ascribe the movements of *Hedysarum gyrans;* the irritability of the *Mimosæ;* the spiral ascent of the twining stem, as being directed to the right or to the left respectively, and never otherwise; the phenomenon of the sleep of plants; and, perhaps, of the *Horologium Floræ?* and how shall we account without it for the adaptation of the vegetable structure to the wants of the species, as exemplified in the hollow stems of the grasses, interrupted with knots; and the hollow but knotless scape of the onion inflated in the middle; together with the growth and maturation of the leaves, flowers, and fruit, which are formed complete in all their parts, and arranged in the most appropriate order, long before their ultimate evolution, and totally independent of gravitation, or of the position in which art or accident may happen to have placed them, or of any other cause that is merely either chemical or mechanical? But if gravitation is really the agent that gives direction to the root and stem of plants, then, I presume, there will be no absurdity in inquiring, whether the upright growth of the horns of the stag, and the twisted and spiral growth of the horns of the ram, are not the effect of gravitation also; or whether the teeth of the upper jaw of a man do not grow downwards, and the teeth of the under jaw upwards, by virtue of gravitation.

I am ready to acknowledge Mr. Knight's great merit in the introduction of several important horticultural improvements, as well as in the discovery, or rather in the more complete establishment of certain important phytological facts; but I do not think that he has been equally successful in the establishment of the several hypotheses which he has advanced for the purpose of explaining the phenomena of vegetation. Perhaps my opinion may be singular, but it has not been formed without much examination, especially on the subject of the present hypothesis, which, I think, I have proved to be not only contradicted by the result of Mr. Knight's own experiments, as well as by a multiplicity of well-known facts; but even indebted for its plausibility to a misapprehension of facts. I am, Sir,

Your most obedient humble servant,

P. Keith.

ARTICLE III.

On the Phenomena of Sanguification, and on the Blood in general. By W. Prout, M.D.

(*Continued from p.* 25.)

Sanguification.—The chyle from the thoracic duct proceeds into the sanguiferous system, mixes with the general mass of circulating fluids, and almost immediately passes through the lungs, where it is exposed to the air, and appears to undergo the final process, and to be converted into blood. This process is termed *respiration;* the phenomena of which we shall briefly consider under the following heads of inquiry.

First, Whether the phenomena of respiration be the same in *kind* in *all* animals.

Secondly, Whether any other gas can be substituted for oxygen in respiration.

Thirdly, Whether the phenomena of respiration be the same *in degree* in different classes of animals compared with one another, or in different animals of the same class.

Fourthly, Whether the phenomena be liable to any differences *in degree* in the same animal at different times.

Fifthly, Whether the blood as *a whole,* or *in part* only, be concerned in the production of these phenomena.

First, With respect to all the more perfect animals which have organs of respiration, &c. similar to man, it need only be stated generally, that precisely the same appearances take place. In the inferior animals, some variations occur which it will be proper to notice. Fishes, for example, have no lungs, and do not breathe air; it was, however, an early discovery, which has been confirmed by all succeeding experimentalists, that these animals cannot live in water deprived of air, at least of oxygen, or more properly speaking, they all require oxygen to be brought in contact with their blood, which oxygen is converted into carbonic acid precisely as in the animals which breathe air. This change is most usually effected by the gills, which are in fact their lungs. In some instances, however, it appears to take place differently, as, for example, in the *cobitis fossilis,* in which a sort of double respiration has been observed by Erman. "In water containing air, the fish breathed as usual through its gills; but if the water was deprived of its portion of oxygen gas, the fish rose above the surface, drew air through its mouth, and swallowed it. The air penetrated the intestines, the blood-vessels of which were reddened; and when it had lost its portion of oxygen gas, the fish discharged it by the rectum." It has [illegible] been lately shown by Biot, whose experiments have been [illegible] recently confirmed by Configliachi, an Italian professor, a[illegible]ar that the air-bladders of fishes contain oxygen gas, which is usually greater in proportion as the animal inhabits deeper waters,* a circumstance which appears to indicate their use to be somewhat analogous to that of the organs of respiration.† In animals inferior to fishes, the same phenomena occur. Thus it was early observed by Ray, that insects died very soon if the holes or *stigmata* through which the air enters into their bodies were stopped with oil or honey. Derham found also that wasps, bees, hornets, also snails, leeches, &c. soon died under the exhausted receiver of an air-pump; and Scheele and Bergman found that like other animals, they converted the air of the atmosphere into carbonic acid. Vauquelin, however, was the first that made accurate and satisfactory experiments with insects, in which he proved beyond a doubt the accuracy of the above conclusions. This chemist also extended his experiments to the *molusca*, and obtained precisely the same results; as did Spallanzani, and more recently Haussman.‡ Lastly, Sir Humphry Davy found that even the zoophytes produced similar phenomena.§ Thus then it appears, that all animals convert the oxygen of the atmosphere into carbonic acid gas; and as the blood is the fluid which appears to be operated upon, and to produce this remarkable change in the more perfect animals, we may doubtless conclude that a similar fluid, or one that performs a similar office, is the cause of this change in the inferior animals, although we cannot discover its existence.

Secondly, We come to consider whether any other gas can be substituted for oxygen in respiration. This question was very early decided in the negative. It was also found that animals could not respire even oxygen for any length of time without dilution, and in short that no other compound, except atmospheric air, in which the proportion of oxygen is only one-fifth of the whole bulk, is capable of supporting life. Pure oxygen and gaseous mixtures containing a larger proportion of this gas than atmospheric air, appear to destroy life in a short time by over excitation. On the contrary, some gases of a mild and inactive character, as hydrogen and nitrogen, when pure, or in too large proportion, destroy life by the opposite means, or suffocation; while others, as carburetted hydrogen, carbonic acid, &c. seem to prove fatal simply in virtue of their deleterious properties.

Agreeably to what might be expected are the effects which these different non-respirable airs produce upon the blood out of the body; no gas, except oxygen, or those compounds containing it in a free state, gives to blood that fine florid colour which it possesses in the arteries, and which appears essential to render it capable of performing its important offices. Some act upon it chemically and decompose it; while others, without exerting any very evident chemical action, appear nevertheless to render it of a darker colour than venous blood itself. A question has arisen among physiologists, whether the nitrogen entering into the composition of atmospheric air be absorbed, or otherwise altered in respiration, and consequently whether it be of any further use in that function than merely acting as a diluent to the oxygen. The most common opinion at present is, that it is not absorbed in respiration. Some physiologists, however, are of a different opinion, and maintain that under certain circumstances, it is absorbed in considerable quantity. The matter, therefore, at present may be considered as *sub judice.**

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* See Berzelius's View of the Progress and Present State of Animal Chemistry, p. 44. Also *Annals of Philosophy*, vol. v. p. 40.

† See an excellent paper on the respiration of fishes, recently published by MM. Provençal and Humboldt, in Mem. d'Arcueil, ii. 259. Those gentlemen found that fishes not only convert oxygen gas into carbonic acid gas, but that a considerable proportion both of oxygen and azote is absorbed during the respiratory process.

‡ See Johnson's History of Animal Chemistry, vol. iii.

§ See Davy on Respiration, in Beddoe's Medical Contributions.

Thirdly, We come to inquire whether the changes which take place in respiration differ *in degree* in different classes of animals compared with one another, or in different individuals of the same class. On this part of the subject, good experiments are much wanting. With respect to the first consideration, we can only speak generally. None of the more perfect animals are capable of existing, even for a few minutes, without oxygen; while many of the inferior ones can exist for a considerable time upon very little. Birds, from the magnitude of their lungs, and some other circumstances of their economy, are supposed, generally speaking, to require most oxygen, and next to them the mammalia; but I am not aware that any comparative experiments have been made upon the subject which we can rely upon. In both these classes of animals, however, the difference between the venous and arterial blood in point of colour is very striking.

Fishes, from the circumstances of their situation, must consume much less oxygen than either of the above classes; and frogs, toads, and other animals of this class have been found to live much longer in a given quantity of air than birds or small quadrupeds of an equal size. The differences also in point of colour between the venous and arterial blood of those animals which require little oxygen, are stated to be very trifling, and almost imperceptible. Vauquelin found that insects of the grasshopper tribe generally died before the whole oxygen of the vessel in which they were confined was consumed; while other insects, as the bee, are stated to consume the last particle of this gas. From Vauquelin's experiments also this appears to be the case with snails and other *molusca,* to such a degree indeed, that this chemist even recommends their use as an eudiometer, or means of separating the whole of the oxygen from a mixture into which this gas enters.* The changes produced upon the fluids or blood of these animals are unknown. As to the second consideration, whether different individuals of the same class differ in the degree of their respiratory powers, we have likewise no very good experiments; and even those we have, from want of due attention being paid to circumstances which materially influence their results, and which will be considered more fully under the next head, can hardly, perhaps, be fairly compared with each other. In a paper which I published some time ago on respiration, I collected the results of all the chief experiments on record, and arranged them in the following tabular form; and although they do not show the exact degree in which individuals differ from one another, they demonstrate beyond a doubt that such differences *do* exist.†

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* For the best observations on the effects of the different gases in re
Davy's Researches on Nitrous Oxide.

	Cubic Inches.
M. Jurine of Geneva "imagined" that for every 100 cubic inches of atmospheric air respired, there were given off of carbonic acid	10·00
Goodwin estimated the quantity at	10·00 or 11·0
Menzies, from experiments made with considerable accuracy, at	5·00 or 5·1
Lavoisier and Seguin appear to have made it much less, especially in their later experiments. From the data in my possession, I am unable to ascertain the precise proportion.	
Dr. Murray found it vary from	6·20 to 6·5
Sir H. Davy, from	3·95 to 4·5
Messrs. Allan and Pepys, from 3·50 to 9·50 per cent. according as the first or last products of an expiration were tried. They estimated the mean at about	8·00
Myself, from 4·1 to 3·3. Mean of the 24 hours about	3·45
A friend, about	4·60
Dr. Fyfe,‡ about	8·50

Now it will be proper to observe, that it has been estimated that the lungs of an ordinary man contain about 280 cubic inches, one seventh of which, or 40 cubic inches, is drawn in and expelled at every inspiration and expiration, the number of which inspirations and expirations in one minute has been estimated at about 20.§ Hence, such a man will breathe about 28,800 times in 24 hours, and take into his lungs, during that period, 1,152,000 cubic inches of atmospheric air; and says Berzelius, on the data of Messrs. Allan and Pepys, which are supposed to be the most accurate, will elicit from his lungs upwards of 11 oz. of carbon. The quantity of water discharged from the lungs during the same time has been estimated at about 20 oz.* With respect to the inferior animals, I am aware of no experiments made upon different individuals of the same class that we can compare with one another.

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* Annales de Chimie, vol. xii. p. 273.

† See *Annals of Philosophy*, vol. ii. p. 333.

‡ See Dissertatio inauguralis de copia acidi carbonici e pulmonibus inter respirandum evoluti, p. 11.

§ See Bostock on Respiration. Also Thomson's Chemistry, vol. v. article Respiration.

Fourthly, We have to consider whether the phenomena of respiration be liable to any differences in degree in the same individual at different times; and to this part of our subject no one, except Dr. Fyfe and myself, seems to have much attended. Our experiments, however, have led us both to the same conclusion; namely, that the quantity of carbonic acid gas found in the lungs is liable to be very materially affected in its quantity in the same individual by various circumstances. These variations in quantity may be considered as of two descriptions, viz. *general* or *diurnal* variations, and *particular* variations. With respect to the first, all my experiments have tended to show, that the quantity of carbonic acid gas formed by the lungs is greater during the day than the night, and that the quantity begins to increase about day-break, and continues to do so till about noon, and afterwards decreases till sun-set. During the night it seems to remain uniformly at a minimum. The maximum quantity given off at noon, I have generally found to exceed the minimum by about one-fifth of the whole. Different days, however, differed in all these respects; and from causes of which I am at present entirely ignorant. Mr. Brande states, that he has found the quantity given off to be greater towards night,† but I have not observed this. As to particular variations, it appears that there are many more circumstances which have a tendency to diminish the quantity than to increase it; and that wherever it has been inordinately raised or depressed, either above or below the standard, it is subsequently, in a certain degree, depressed or raised above the standard, thus preserving upon the whole a constant mean quantity. The passions of the mind appear to have a great influence over the quantity; those of a depressing kind, diminishing it, and those of the opposite nature, the reverse. Exercise, when moderate, appears to increase in some degree the quantity; but violent and long continued exercise diminishes it. The greatest decrease experienced was from the use of alcohol and vinous liquors in general, especially when taken upon an empty stomach. In short, whatever diminishes the powers of life, as low diet, mercurial irritation, &c. appear both from Dr. Fyfe's experiments as well as my own, to have the effect of diminishing the quantity. The quantity is also apparently much diminished during sleep. Some are of opinion that there is more carbonic acid given off a few hours after eating, and when the chyle may be supposed to be entering the sanguiferous system, but I have not myself observed this circumstance. With respect to these observations in general, I am fully aware that they are too limited and imperfect to be much relied upon, though I am persuaded that if this part of the subject were duly investigated, it would throw a great deal of light on this obscure function. Imperfect as they are, however, they are sufficient to account in some degree for the very great differences in the quantity of carbonic acid gas, said to be given off by different individuals, as stated in the preceding section. As far as I am aware, no similar experiments have been made on the inferior animals.

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* Berzelius's View of Animal Chemistry, p. 39.

† Phil. Trans. 1809. Nicholson's Journal, vol. xxv.

Fifthly, We come to consider whether the blood as a whole, or in part only be concerned in the production of these phenomena. This question cannot be easily decided by experiment. It appears, however, from some observations of Berzelius, that the colouring matter of the blood is the principle from which the carbon is chiefly derived in respiration. "It has been generally believed," says this accurate observer, "that every part of the blood is influenced by the air; that it absorbs oxygen, and exhales carbonic acid gas; but this is not the case. Blood, in which the colouring matter is still contained, absorbs oxygen gas very quickly when out of the body, and shaken in atmospheric air; it also retains at the same time some part of the carbonic acid thereby produced; on the other hand, serum, when destitute of colouring matter, does not change the atmospheric air before it begins to putrefy." * The colouring matter, however, appears to possess this property in its natural state only; and whilst it is in contact with the other principles of the blood; for if it be separated and diluted with water, it seems no longer capable of being affected by the contact of atmospheric air; at least, it undergoes no change in colour.† This is a very important fact, and deserves to be better investigated.

We come now to consider a little more closely the phenomena and nature of these mysterious processes, by which substances foreign to animal bodies are assimilated to their nature.

The nature of the digestive process has engaged the attention of physiologists from the earliest times; and the aid of all the various physical agencies and sciences which happened to occupy the attention of philosophers at the time, has been successively called in to explain its phenomena. By Hippocrates it was attributed to a sort of concoctive fermentation. By Galen and his followers, chiefly to heat. By Helmont, to his archæus. By the Jatro-mathematici, to trituration. By Pringle and Macbride, to fermentation. And, lastly, by Hunter, Spallanzani, and most physiologists since their time, to the agency of a peculiar

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* See Berzelius's View of the Progress and Present State of Animal Chemistry, p. 36.

† See Observations and Experiments on the Colour of the Blood. By W. C. Wells, M.D. F.R.S. Phil. Trans. 1797, Part II.

fluid, secreted by the stomach, and denominated the *gastric juice*, the properties of which will be first briefly considered.

From want of proper attention being paid to the heterogeneous nature of the fluids found in the stomachs of animals, great confusion has arisen in the description of their properties. Fordyce indeed long ago pointed out the necessity of attending to this circumstance, but many of his successors have not much profited by his observations. The fluids of the stomach may be considered as arising from at least four different sources, each of which furnishes a distinctly different fluid. These are the salivary glands, the mucous coat and exhalents of the stomach itself and the passages leading to it, and lastly, the gastric glands, which alone indeed seem to furnish the true gastric juice or fluid, which appears to perform so important an office in the function of digestion. The saliva of different animals of course must be very different. That of man, according to Berzelius, contains, like most other products secreted by glands, no albumen, but a peculiar animal matter, some mucus derived from the mucous membrane of the mouth, &c. and the usual salts of the blood, all dissolved, or rather, perhaps, suspended in much water. The mucus derived from the mucous membrane of the stomach appears to resemble closely that of the mouth and pharynx. The fluid separated by the exhalents appears to consist, like that fluid in general, of little more than water holding in solution the salts of the blood. The properties of the fluid secreted by the gastric glands are unknown, it never having been obtained in a separate state. Its characteristic property in all animals seems to be that of coagulating milk.* These different fluids then, with often a portion of bile, are always found mixed together in the stomachs of animals, and of course at different times in very different proportions. Thus from the stomach of a dog I have sometimes obtained a limpid and nearly transparent fluid, incapable of coagulating milk when assisted by the most favourable circumstances, and apparently consisting of little more than water. At other times I have obtained a fluid capable of coagulating milk very readily.

A question strongly agitated among physiologists has been, whether the fluids of the stomach are naturally acid or alkaline. Spallanzani maintained that they are naturally neutral, and this opinion appears to be most probable; though the contents of the stomach, when the digestive process is going on, are almost always acid. The nature of this acid I have not been able to ascertain in a satisfactory manner. By some of the older chemists, it was asserted to be the phosphoric. M. Montegre says, it is the acetic.† It is evidently some volatile acid, from its effects on litmus paper, being so very evanescent. I considered it in the pigeon as the carbonic. There appears, however, to be occasionally another acid, which is of a much more permanent nature, and is probably the phosphoric acid—a circumstance that has very likely contributed to the above-mentioned diversity of opinion on the subject. In the fluids of the stomachs and alimentary canals of all animals that I have examined, I have uniformly observed distinct indications of the presence of lime in some slight state of combination. It may be separated by digesting a portion of the alimentary matters in acetic acid, and adding oxalate of ammonia to the solution obtained. A copious white precipitate takes place, which consists of oxalate of lime in union with some animal matter, probably mucus, which, in almost all instances, appears to contain lime either in some peculiar state of combination, or perhaps of mixture.

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* See experiments to ascertain the coagulating power of the secretion of the gastric glands. By Sir E. Home. Phil. Trans. 1813. Part I.

† See a short accouut of the experiments of M. Montegre in the [illegible] of the Royal Institute of France, for 1812.

The fluids of the stomach are stated by Spallanzani and others to possess strong antiseptic powers both out of the body as well as in it. Thus, according to Spallanzani, pieces of meat can be preserved in them for a long time out of the body without putrefaction; and a piece of putrid meat, it is said, even becomes sweet in the stomach of a dog in a short time. This latter circumstance, perhaps, arises in part from the putrid portions being already in a half decomposed state, and thus more readily dissolved than the sound parts which are left. M. Montegre denies most of the above observations, and concludes that the gastric fluids do not differ from saliva; that they cannot stop putrefaction nor produce digestion independently of the vital action of the stomach, and that the acidity which appears, arises from the food during the digestive process, and is the effect of the action of the stomach; of these conclusions, however, the first is certainly erroneous. Some idea of the *quantity* of the gastric fluids may, perhaps, be formed from the fact formerly stated, that upwards of half an ounce of fluid was pressed from the contents of the stomach of a rabbit fed on perfectly *dry* food.

The contents of the stomachs of animals feeding on vegetable substances, even when apparently fully digested and about to pass the pylorus, exhibit no traces of an albuminous principle; the moment, however, they enter the duodenum, they undergo a remarkable change, not only in their appearances, but their properties. These changes appear to be chiefly induced by the action of two secreted fluids with which they there come in contact, and are intimately mixed. These are the bile and pancreatic juice, on the nature of which we shall make a few remarks. The bile consists chiefly, according to the accurate observations of Berzelius, which agree with my own, of a large proportion of water holding in solution a peculiar bitter substance, named the biliary principle—of the mucus of the gall bladder, and of the usual salts contained in the blood and in all the fluids secreted from it. The properties of the pancreatic juice I never could satisfactorily ascertain; but it has usually been considered as analogous to the saliva; and if this opinion be correct, it may be safely considered as containing no albumen. The changes produced in the digested alimentary matters by these fluids are evidently of a chemical nature. A gaseous product is usually evolved; a distinct precipitation of the biliary principle in apparent union with some others, chiefly of an excrementitious nature, takes place; the mixture becomes neutral; and *an albuminous principle is formed*, at least, traces of this principle appear, which, however, become much more distinctly visible at some distance from the pylorus.* I tried to produce these changes out of the body, and with this view mixed a portion of the fluid obtained from the contents of the stomach of the rabbit, formerly described, with a portion of the bile of the same animal. A distinct precipitation took place, and the mixture became neutral; but although I thought that the resulting fluid was more of an albuminous nature, yet the formation of a perfect albuminous principle was doubtful; probably the presence of the pancreatic juice was necessary to complete the formation of this principle.† The proportion of this albuminous principle, after a certain distance from the pylorus, decreases rapidly as we descend the alimentary canal, and at length nothing is left but excrementitious matters, consisting chiefly of the undissolved parts of the food, combined with the mucus of the intestines, and the biliary principle somewhat altered in its nature. Further changes, the nature of which is not very well understood, take place in these matters, more especially in the cœca and large intestines of those animals which feed on vegetable substances. Here it is they assume the usual excrementitious appearance. Some think that various matters, noxious to the economy, are excreted here; while others consider this part of the alimentary canal to be a sort of secondary stomach, intended to digest those substances which escape the solvent power of the first. Both these opinions may be in part correct.

The phenomena of chylification have been still less satisfactorily observed than those of chymification. It seems, however, to be placed beyond a doubt that the proportion of albuminous matter, and especially of fibrin, is much less; or at least their principles exist in a much less perfect state in the chyle as immediately taken up from the intestines, than as it exists in the thoracic duct, and about to enter the sanguiferous system. A portion of these albuminous principles, therefore, is evidently either formed altogether, or its formation is completed during the passage of the chyle through the lacteal vessels. Perhaps the last view of the subject is most probable, and it has accordingly been in conformity with this view that I have ventured to call by the name of *incipient* albumen a peculiar principle uniformly found in the chyle of the mammalia, and which appears to decrease in quantity as the two albuminous principles increase. Concerning the nature of this principle, various opinions have been entertained. One of the oldest and most common has been, that it is similar to the caseous principle of milk, and the chyle in consequence was long considered as analogous to milk in its properties. What makes the resemblance still more striking is, that in chyle an oily or butyraceous fluid is very often present, which, rising to the top of the serum, in conjunction with the caseous-like principle of which we have been speaking, form an appearance exactly resembling the cream of milk, and these principles are often so abundant, especially in the chyle of animals fed on flesh, that, as Dr. Marcet has observed, they may be very readily detected even in the blood itself. Vauquelin remarked the near resemblance of this fatty matter to that which he extracted from the brain; and I had made the same remark before I had seen Vauquelin's paper, not indeed with respect to the fatty matter (for I believe none exists naturally, in the cerebral mass of the mammalia at least), but with respect to the peculiar matter which has been compared to the caseous part of milk, and which certainly very closely resembles in its chemical properties the substance of the brain. Hence I once thought it likely that this principle was designed to form the cerebral and nervous substance; but this opinion I must confess is founded on very slender grounds, and the probability is much greater that it is nothing but the albuminous contents of the blood in an imperfect state.

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* It is true that in two of the specimens of chyme, that, namely, of the dog fed on vegetable food, and that of the ox, described in a former part of this paper, no traces of albumen were obtained. From my not having collected either of these specimens myself, I cannot be supposed to be able to account satisfactorily for this circumstance. In the first instance, the dog had been fed on a species of food which was unnatural to him, and the quantity of albumen was small, even in the chyle of the same animal; probably, therefore, the whole had been taken up from the intestines. In the other instance, the chyme had been kept for some time before I examined it, and, besides, seemed to contain an unnaturally large proportion of bile. My not finding albumen in these substances occasioned me to consider its presence to be much less general than I have since found it, and to be probably confined to the carnivorous animals.

† I by no means wish to be understood to assert, th[illegible]e and pancreatic juice are the sole agents operating to produce this chan[illegible]ital action of the duodenum itself must be probably taken into account.

But it will be doubtless asked, if albumen be formed in the duodenum, why it is not *all* formed there. To this it may be answered, that the formation of albumen appears to require a *certain time to be completed;* for I have uniformly found the greatest quantity of albumen not immediately below the pylorus, where we might expect to find it if its formation were instantaneous, but at some distance further down: we may, therefore, conclude with Dr. Marcet, that in those animals whose food is productive of a great deal of chyle, and especially in carnivorous animals, this fluid is taken up by the lacteals, and even sometimes reaches the blood, before it can actually be converted into albumen; but that this change nevertheless takes place afterwards either in consequence of the original tendency given to it in the duodenum, or of the subsequent action of the absorbent vessels, &c. through which it passes. If it be objected as unlikely that the lacteals should take up such imperfectly formed and crude materials, it may be answered that they often take up substances much more dissimilar to those which are natural to them, as has been often found by actual experiments made with musk, colouring substances, &c. and indeed, as is sufficiently proved by daily experience, with medicinal substances, many of which do not appear to operate till taken into the mass of blood.

My readers will doubtless remark, that I have not mentioned the existence of fibrin and the red particles in the duodenum, which ought to be the case, provided the original notion, stated at the commencement of this essay, were well founded. To this I answer, that although I never could completely satisfy myself of the actual existence of fibrin in the duodenum, yet I often noticed that its contents underwent a distinct and remarkable change on exposure to the air, and which appeared analogous to that sort of dissolution which we stated the coagulum of chyle to undergo when placed in similar circumstances; that is to say, from being generally of a glairy and rather firm consistence, they became, after an hour or two, thin and ichorous. That fibrin, however, is occasionally, if not always, formed in the duodenum, is very probable, from its being found in the lacteals immediately after it has been taken up from the intestines. Some indeed may feel inclined to attribute its formation to the act of absorption; but from what has been said above, it seems very likely that this is little else than a mechanical process. With respect to the red particles, they certainly do not exist as red particles in the duodenum, nor even, perhaps, in the chyle itself; *white* particles, however, are found in the chyle at a very early period of its formation, and these, in part at least, appear to have the property of becoming red on exposure to the air; for chyle, as we formerly stated, assumes a pinkish hue after it has been removed some time from the thoracic duct. These white particles, therefore, are probably the same as the red particles, the red colour not being developed (at least completely) till they have been exposed to the action of the air in the lungs. There is, however, evidently another variety of *white* particles in the chyle besides those destined to become the future *red* particles. These are much larger, and appear to be formed of the caseous-like and oily principles stated to exist in chyle, and which are insoluble in the serous portion and therefore, naturally assume the globular form, like oil [illegible] through water.

Lastly, We have to consider the mod[illegible] of those agents which operate in the production o[illegible]enomena of assimilation; and upon this part [illegible]ust be confessed our knowledge is lamentably deficient. The chief object of the digestive process appears to be to produce an aqueous solution of the alimentary matters; and the chief agents which operate in producing this solution appear to be the fluids of the stomach; but *how* these agents operate, very little is known.[*] Their operation, however, appears to consist, in part at least, in combining with the food, and thus forming a *tertium quid* different from either, though partaking of the nature of both; for all the phenomena seem to warrant the conclusion, that the gastric fluids form a *necessary* part of the chyme, and thus ultimately, perhaps, of the blood itself. The nature of the operation of the bile and pancreatic fluid has formed a fertile source of conjecture to physiologists from the earliest times. To mention all the opinions that have been held on this subject would be worse than useless. Boerhaave maintained, that its chief use is to correct the acidity of the digested mass as it passes from the stomach into the duodenum; and in all the instances which I have witnessed, the acid digested aliments have been rendered neutral on mixture with the bile. Whether this be a constant effect, I cannot say. The biliary principle does not appear to enter into the chyle, as has been long observed by physiologists, but other principles of the bile do; among these, perhaps, is the alkali which it contains, and which is very probably the source of that alkali which exists in some slight state of combination in the blood. The presence of bile, however, does not appear to be a *sine qua non* in sanguification, as this process goes on to a certain extent, when the *ductus communis choledochus* seems to be completely obstructed by biliary concretions, or even when secured by a ligature if we can believe Fordyce. The nature of the operation of the pancreatic fluid is entirely unknown, as is that of the lacteal vessels and glands conneeted with them. Some have supposed that the glands secrete a fluid, which mixes with the chyle in its passage through them—that they ordinarily produce some change in the chyle appears to be evident from the fact which has been long observed, that this fluid passes from them less white and opaque than when it entered them. The chyle in its passage towards the thoracic duct becomes mixed with the fluids brought by the lymphatics from all parts of the body, which fluids, if they exert no other action upon it, must at least have the effect of rendering it much more animalized, if I may use the expression, and thus of counteracting the ill effects which a crude fluid like the chyle would probably produce on the system by passing undiluted into the blood.

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[*] When the *par vagum* is divided, the digestive process is said to be suspended. Admitting this, some will be inclined to explain it on the general principle, that secretion is the effect of nervous action, and that in the present instance, the secretion of the fluids requisite to digestion is suspended. Dr. Wilson Philip has lately attempted to show, that galvanism may be substituted for nervous action in this instance.

With respect to the intimate nature of respiration, we are almost as ignorant as of that of the other steps of the assimilating process. Is the carbonic acid given off as carbonic acid by the blood, and an equal volume of oxygen gas absorbed; or is the carbon only given off, which, by combining with the oxygen of the atmosphere, forms the carbonic acid? With respect to this important point, physiologists have differed much in opinion. Some, as Hassenfratz, and Lagrange, supposed that the oxygen penetrates the delicate vessels of the lungs, remains in the arterial blood in a state of solution or loose combination, till it reaches the capillaries, where it passes into more intimate combination with carbon, and thus forms carbonic acid, in consequence of which, the blood passes into the venous state, and that this carbonic acid lies dormant in the venous blood till it reaches the lungs, where it escapes in a gaseous form, and a new portion of oxygen is absorbed. The most common opinion, however, is, that the carbonic acid gas is formed in the lungs by the union of the carbon of the blood with the oxygen of the atmosphere, though physiologists differ as to the precise mode in which this union takes place, some supposing that the oxygen actually penetrates the delicate membrane lining the lungs, and forms the carbonic acid *within* the vessels; and others, especially Mr. Ellis, contending that the carbon escapes through the same membrane, and combines with the oxygen *without* the vessels.* As to the opinion, that the oxygen is not absorbed into the blood, but that the carbonic acid gas is formed in the lungs, it is certainly by far the most probable in the present state of our knowledge. We know, for example, that oxygen gas on being converted into carbonic acid gas, is not changed in volume; and, as before observed, the most accurate experiments on respiration appear to show, that during this function, a volume of oxygen ordinarily disappears, precisely equal to that of the carbonic acid gas formed† —a fact which it is extremely difficult to account for on any other supposition; for it is very unlikely that this coincidence in volume should so uniformly occur, if the phenomena were not more intimately connected as cause and effect than they would necessarily be, on the supposition that the carbonic acid is given off from the blood as carbonic acid, and the oxygen absorbed.

With regard to the particular manner in which the carbonic acid is formed, whether internal or external to the vessels, I confess I have no decided opinion. It seems most probable that the carbon is excreted, perhaps in a state of solution in the watery vapour which is elicited from the blood, and that it combines with the oxygen of the air at the moment it escapes from the exhalents. For it is not easy to conceive, under the circumstances in which the lungs are placed, how oxygen can be passing in and vapour passing out through the same membrane at the same time. Further we learn, from M. Majendie's experiments, which have been repeated with success by M. Orfila, that phosphorus dissolved in oil, and injected into the jugular vein of a dog, is expelled by the mouth and nostrils in the form of copious vapours of phosphorous acid,* which could hardly have been the case if the phosphorous acid had been formed within the vessels; as in this case, we should have supposed it would have remained in solution in the blood, it not being a volatile substance. We may, therefore, suppose, that the phosphorus was excreted in a state of minute division from the vessels of the lungs, and meeting in this state with the oxygen of the atmosphere, formed the phosphorous acid in question; and if this reasoning be admitted with respect to phosphorus, I cannot see why it should not be admitted with respect to carbon.

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* See Ellis on Respiration.

† See Experiments on Respiration, by Messrs. Allan and Pepys, Phil. Trans. 1808 and 1809.

It has been supposed, as before mentioned, that one *use* of respiration is to convert the chyle into blood, which process is stated to be effected by the removal of redundant carbon; and it has been maintained in support of this opinion, that more carbonic acid gas is given off when the chyle is supposed to be entering the blood. Admitting this use of respiration, the manner stated cannot be that in which the change in question is effected; for if it were, animals, after long fasting, and when there was no chyle to assimilate, might be supposed to emit little or no carbonic acid, and in short to be able to do without respiration, which is contrary to observation; besides, many animals after eating naturally sleep, in which state it is generally acknowledged, that little carbonic acid is given off; but if we even admit that more carbonic acid is given off after eating, which to a certain extent may be true, this fact may, perhaps, be better explained upon other principles. What then is the real nature and use of respiration? Does nothing take place in this function but the separation from the blood of a little superfluous carbon? If this were its only use, why are precisely the same processes uniformly adopted? Could not this carbon be got rid of equally well in various other ways, as, for example, in the form of carburetted hydrogen, &c.? Why is oxygen always necessary, which apparently never enters the economy, but is instantly expelled under the form of carbonic acid? These obvious questions have been often asked; and physiologists have puzzled their brains to discover a result more adequate to a process, so important in the animal economy as respiration; but after all, their labours have not been very successful. Many theories have indeed been formed on this subject, and till lately, one of them, which supposed animal heat to be the result of the respiratory process, was pretty generally admitted; but as the data upon which this pretended explanation was founded have been recently controverted, the whole fabric must fall to the ground. Still, however, it appears indubitable, that both the assimilation of the chyle and the formation of animal heat are intimately connected with respiration, though from the vital character of the processes, we shall probably ever remain ignorant of their precise nature.

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* See Experiences pour servir à l'Histoire de la Transpiration Pulmonaire: Mémoire lu à l'Institut. de France, en 1811, p. 10. Also Orfila's Toxicologie *Genérale*, tom. i. Part II. p. 189.

(*To be continued.*)

Article IV.

Origin of Steam-Boats, and Description of Stevenson's Dalswinton Steam-Boat. By a Civil Engineer. (With a Plate.)

Among the various important mechanical uses to which Mr. Watt's improvements on the steam-engine have enabled us to apply steam as a power, that of propelling vessels without the aid of winds or of tides, not only in rivers, but in large friths and arms of the sea, is none of the least; and it is only the difficulty of procuring a sufficient supply of fuel which now prevents it from being extended generally to the wide expanse of the ocean.

It is somewhat more than a century since the first invention of the steam-engine, by Savary and the Marquis of Worcester. It was afterwards improved by Newcomen, who, in conjunction with Savary, obtained a patent for its invention and improvement in the year 1705. About 1712, it appears to have been first used for pumping water at collieries; and before 1720, it had come into pretty general use. In 1725, a *fire-engine* was erected at the collieries of Edmonstone, which was probably the first upon Newcomen's plan, which was erected in Scotland.

The early history of the steam-engine has been pretty carefully ascertained, and is generally known; but what we wish to establish in the present case is, the period at which the steam-engine was first employed afloat as the propelling power, in order that we may be enabled to claim it distinctly as a *British invention.* Accordingly it appears, in the year 1736, that Jonathan Hulls, of London, obtained a patent for the invention of a steam-boat engine, which will be found among the list of British patents for that year. In the year following, Mr. Hulls published a pamphlet upon his invention, to which he has given the following title, viz.: "A Description and Draught of a new-invented Machine for carrying Vessels or Ships out of or into any Harbour, Port, or River, against Wind and Tide. or in a Calm; by J. Hulls, London. Printed for the A[illegible] 37. Price 6*d.*" In this pamphlet, various problems in [illegible] ties are commented upon, by which the operation of [illegible] illustrated. But these are more applicable to the early knowledge of that engine than to the present times; and, therefore, we shall rather follow him in his description of the mechanism of his steam-boat and engine in the following terms:

"In some convenient part of the *tow-boat*, there is placed a vessel two-thirds full of water, with the top clogs shut; this vessel being kept boiling, rarefies the water into steam; this steam being conveyed through a large pipe into a cylindrical vessel is there condensed, and makes a vacuum, which causes the weight of the atmosphere to press on the vessel, and so presses down the piston that is fitted into this cylindrical vessel in the same manner as in Newcomen's engine."

"It hath already been demonstrated, that upon a vessel of 30 inches diameter, which is but 2½ feet, when the air is drawn out, the atmosphere will press to the weight of 4 tons 16 cwt. and upwards; therefore, when proper instruments for work are applied to it, it must drive a vessel with great force."

We have distinctly here the application of the steam-engine in 1736 as a propelling power to a vessel afloat, or in other words, the discovery of the steam-boat; and although, as far as we know, the inventor confined his views to the navigation of rivers and the entrance of harbours, yet it is easy to see how it might and has been extended to friths and arms of the sea, and may be extended to more distant voyages.

We find accordingly that the late Patrick Miller, Esq. of Dalswinton, in Scotland, in the course of his various and ingenious investigations into the proper mechanism and sailing of ships, constructed some vessels with double and triple keels, to be worked with sails, and also with a steam-engine. By a letter from Mr. Miller to Mr. George Salmond, of Glasgow, dated Jan. 12, 1815, it appears that Mr. Miller was employed in these pursuits prior to the year 1787, when he wrote a treatise, of which he presented copies to the following illustrious personages: to use his own language, "in the first place, to our King, also to the late King of France, to the Emperor of Russia, to Holland, the Kings of Sweden and Denmark, and other Sovereigns: I also sent copies thereof to the President of America, Mr. Washington, to the then Ambassador from America to our Court at London, and also to Dr. Franklin. Of this treatise, I also sent a copy to the Advocates' Library, and another to the University of Edinburgh, and to the Universities of Cambridge and Oxford, and the Royal Society at London."

Mr. Miller also made various experiments about that time on the Forth and Clyde canal, with a boat fitted up with a steam-engine;* and he mentions, in the letter above alluded to, that these experiments succeeded. The late Earl of Stanhope, famous as a mechanical philosopher, laboured for years with the steam-boat at his seat of Chevening, where he afterwards tried many experiments upon a lake in his grounds. The experiments by Mr. Miller on the Forth and Clyde canal, we have been informed, were either seen by, or communicated to, the late Mr. Fulton, engineer of America, who, it is believed, was a native, at least resided in this part of Scotland, but afterwards went to America, where he had the merit and the honour of introducing the steam-boat upon an extensive scale on the great rivers and lakes of that country; so that we can trace this invention most indisputably to a British origin.

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* *The steam-engine* in Mr. Miller's boats was employed to turn a wheel pre*cisely as is practised* at present in steam-boats.

It is not a little remarkable in the history of the arts, and forms a striking instance of the slow and progressive steps by which they advance, that that most elegant and useful discovery, the steam-boat, first brought forward in 1736 by Jonathan Hulls, of London, and afterwards publicly investigated and tried by Lord Stanhope and Mr. Miller, of Dalswinton, should have been carried to America, and there first have changed its character from mere experiment to extensive practice and utility, and that it should again have been introduced into Britain upon the experience of Americans only so lately as the year 1813, when it was first employed upon the river Clyde, by Mr. Bell, of Helensburgh, in Dumbartonshire. From this period, however, it has been extended to all parts of the united kingdom, and to several of the continental states; and though a subject still in its infancy, it will, without doubt, be carried to much greater extent by the discoveries of the ingenious and the adventurous spirit of seamen.

In the steam-boat as now principally used in Great-Britain, and very generally in America, the paddles, or wheels, are placed upon the outside of the gunwales of the vessel, which add much to her breadth, and render her extremely inconvenient and liable to accident in harbours and rivers, especially when these happen to be crowded with shipping. From the circumstance of the extraordinary breadth of the steam-boat, she is not only much impeded, but she is found greatly to hamper the navigation of *narrow fareways,* containing a breadth, in some instances, of no less than 30 feet over all, and must accordingly be greatly exposed to accident in being frequently run foul of by other vessels. The following desiderata seem much to be wanted in the use of the steam-boat: in the first place, that the paddles, or wheels, should be better secured from accident in the ship, and also that she should, without risk, be enabled to *sheer up* or take a *birth* in a harbour alongside of another vessel without exposure to injury.

The arrangement or position of the wheels is also an object of no small importance, and one which has been attended with much difficulty in obtaining the best effects of the power of the engine. A little reflection will be s [illegible] to show, that the present mode of having one engine [illegible] heels in one position of the ship must be extremely [illegible] ttending to the operation of the steam-boat on the Clyde, the Forth, the Thames, and the Mersey, it has been uniformly observed, that the steam-boats are drawn down or made to dip by the *head* or *stern* into the water, according to the position of the wheels, from four to as much as nine inches, which obliges the boatmen to *trim* or ballast with iron at the sternmost point of the boat. This, on the whole, must not only greatly impede the motion and velocity of the boat; but the whole weight and power also of the engine being exerted in one point, a motion is generated throughout the vessel, which is not only unpleasant to the passenger, but must shake the timbers, and be otherwise extremely injurious to the boat. To obviate this, Mr. Stevenson, civil engineer, proposes, as the reader will observe by Plate XCI, to place the two wheels at or near the extremities of the boat longitudinally, instead of transversely as at present, by which the force of the engine will be more equally divided; the vessel will also be kept upon an even keel, in so far as the action or power of the machinery is concerned; and the boat will, therefore, pass through the water with less interruption, and consequently with more velocity. Further, by employing two smaller steam-engines instead of one large one, and placing one of these in each compartment of a *Dalswinton vessel,* the steam-boat may thus be rendered extremely commodious, and admit of being laid out in neat and commodious apartments for passengers nearly to the full extent of the length and breadth of her deck. Such a construction of boat is also well adapted for carrying goods and cattle, &c. on a ferry.

In considering this subject, the writer of this article begs to turn the attention of the reader to the accompanying plate, entitled "Plans and Sections of Stevenson's Dalswinton Steam-Boat, calculated for the Harbour of Leith, and the Locks of the Forth and Clyde Canal;" and here it may be necessary to observe, that the term Dalswinton is introduced with a view to connect the name of Mr. Miller, that ingenious country gentleman, both with the idea of the steam-boat, and more especially with the double boat, of which he appears to have been exclusively the inventor, and which in the opinion of many is peculiarly applicable to the purposes of a steam-boat. It is also necessary to introduce Mr. Stevenson's name, as we are unacquainted with the precise design of Mr. Miller, who seems to have applied the steam-engine to a treble boat. In so far, therefore, as we know, the idea of providing the accommodation for passengers and goods upon deck, with the machinery below, and the wheels placed in the manner proposed in the accompanying plan and sections, is entirely new.

Description of the Plate.

The plan on the plate marked No. 1, exhibi
the water wheels, steam-engines, and boilers,

sents a plan of the hold of the double vessel with the water-course between them. The plan No. 2, shows the accommodation upon deck, consisting of a fore and after cabin, with cooking-house, &c. &c. The section No. 3, represents a longitudinal section of the vessel in the direction of the keel from stem to stern, showing one of the lower holds in which a steam-engine and boiler are fitted up. Over this again are the cabins, with a walk and railing upon the top. Between the hold and the cabins, a space is marked off with strong plate-iron for ventilation, in which a current of air is kept up from stem to stern. Lastly, the cross sections marked A B and C D refer to the corresponding letters on the plans, and exhibit a transverse section, or view of the boat, both above and below the water line. This general plan on which the several parts are marked, it is presumed will easily be understood, by those especially who have had any experience of the steam-boat, without the necessity of multiplying technical references, which are always irksome to the general reader.

This plan of a steam-boat, in so far as comfort and accommodation to passengers, or the conveyance of goods, are concerned, it is humbly conceived will be found to be extremely useful, and deserving the attention of the public; and especially of those whose concerns more immediately lead them to have an interest in the improvement of the steam-boat.

A question will naturally occur to the practical seaman with regard to the strength of such a vessel; while the philosophical observer may wish to be satisfied as to the fitness of this distribution of the wheels for the purposes of speed or velocity. With regard to the strength of such vessel, every one will be satisfied that while afloat she must be perfectly secure, as the water presses upon all sides equally, and must give her great stability. In order also to enable such a vessel to take the ground in a dry harbour, it will be observed from the cross sections marked A B that considerable facility is obtained in framing this double vessel in a strong and substantial manner, particularly at the space between the two water wheels.

On the subject of the velocity of a boat of this construction, the most satisfactory trial is no doubt obtained from experiments upon the great scale. Regarding the full and proper operation of the two wheels, situated as here proposed, there seems to be no reason for doubting. In the example here offered, the water-course is delineated upon a narrower scale than choice would have dictated; but this boat, being intended to suit the lockage of the Forth and Clyde canal, which will not admit vessels of a greater breadth than 20 feet and more than 70 feet in length, the breadth of beam is necessarily confined more than would otherwise have been requisite for the ordinary purposes of navigation, or birthage, even in a circumscribed harbour. An opinión may, perhaps, be entertained, that the water in this course, which is only five feet in breadth, will be apt to gorge in the space between the two wheels, by which the fore wheel would be loaded with *tail water;* but even if this effect should take place in the fore wheel to a certain degree, the after or sternmost wheel will work with greater advantage from the head thus supposed to be collected between the wheels. But when we consider that the vessel is afloat upon a great plane, and that the after wheel operates with the same velocity and effect as the fore wheel, and that even in the example before us they are placed 40 feet apart, it is not easy to conceive how the water is to gorge up in such a situation; it seems more probable that, the two wheels acting in perfect unison with each other, the water in the *wheel course* will preserve a smooth surface, and that the wheels will work with more advantage by being thus secluded and defended from the boisterous waves of the sea, than when exposed to them on the outside of the gunwales. Upon a tract of canal navigation, a steam-boat so constructed will not only pass along with great velocity, but without injury to the banks, which otherwise could not fail to be the case with the steam-boat in common use.

It is believed to be a common prejudice with observers upon the deck of a steam-boat passing through the water, that there is a current leaving the steam-boat as quickly in a backward direction as the boat is making progress forward; but from the following experiment, and others which the author of this article has made, the apparent effect from the *lapping* or undulating appearance of the water, as seen in the work of the steam-boat, is apt to be mistaken for velocity in the water; whereas a little reflection, it is presumed, will convince every one, independently of the following trials, that this must be a deception. In the month of April, 1818, when returning from Inverary to Glasgow in the Argyle steam-boat, by Lochfine and the Kyles of Bute, a distance of upwards of 80 miles, which this boat performed in the course of 14 hours, a good opportunity was afforded of making various experiments with regard to the velocity of the boat, both in sheltered and also in somewhat exposed situations, and in strong currents, both of the tide and of the river. About 50 pieces of birch timber turned into a spherical form, two inches in diameter, were dropped from the vessel into the sea in all possible directions, when it was uniformly found that the balls dropped in the wake of the wheels of the steam-boat had hardly any sensible motion greater than that compared with those thrown at a greater or less distance from the boat; or with those which were dropped into the water by the *rudder case.* This result one might have come to, *à priori*, by asking one's self how the water in such a situation could possibly acquire a velocity from the lapping of the paddles or wheels, there being no *tail race* or declivity to create and keep up such a motion.

This point being established, it seems to be conclusively in favour of disposing of the wheels of a steam-boat in the form of a water-course, as represented in the plan No. 1, and longitudinal section marked No. 3, upon the accompanying plate; and with this in view, we do not hesitate to recommend to the attention of our readers the construction of the boat here alluded to, as well calculated to give much facility in the navigation and birthage of steam-boats, and to render them applicable to canal navigation, for entering harbours, and sailing in rivers, crowded with shipping, with the most, perfect security; while such a degree of convenience and accommodation is afforded to passengers as must render the *Stevenson Dalswinton Steam-Boat* an object of very considerable importance on ferries, and on various distant passages throughout the kingdom; as, for example, upon Queensferry, Kinghorn, and Dundee, and from Leith to various ports on the coast of England, and ultimately to London. The same description of boat is well calculated for passing from Harwich to Rotterdam, from London to Ostend, from Dover to Calais, &c. &c. &c.; such boats are also well calculated to sail from Glasgow and Greenock through all the Lochs of Argyleshire, and the Crinan and Caledonian canals, to the eastern coast of Scotland; also to Belfast, Dublin, and Liverpool, and upon the great public ferries from Holyhead to Dublin, and Portpatrick to Donaghadee. Indeed on many of these passages the steam-boat has already been tried, even in its present imperfect state, with a good effect; and we despair not of seeing this noble invention so much matured that their voyages to the distant parts alluded will be made with such a degree of safety and dispatch as to render this one of the most valuable improvements connected with the insular situation of Great Britain.

Thus if our humble endeavours shall be found useful either in advancing the improvement of the steam-boat, or in tracing these inventions to their proper authors, our end and object will have been completely obtained.

ARTICLE V.

Observations on the Chemical Constitution of Muriatic Acid Gas, and on some other Subjects of Chemical Theory. By John Murray, M.D. F.R.S.E. Fellow of the Royal College of Physicians of Edinburgh.

(Concluded from p. 39.)

ADMITTING water to be procured from [illegible] acid gas in those forms of experiment, direct or ind[illegible] the agency of no other substance that can afford it, is introduced, the conclusion seems necessarily to follow, which forms the basis of one of the two systems under which the relations of oxymuriatic and muriatic acids have of late years been explained, that oxymuriatic acid is a compound of muriatic acid with oxygen; and that muriatic acid in its gaseous state contains combined water. This doctrine, accordingly, may be maintained, and may even perhaps be just. It is not, therefore, from the consideration of any deficiency in its support that I depart from it in the following observations; but that I consider the view I have to propose as perhaps more probable, or at least as, on the whole, according better with the present state of chemical theory. In a science such as chemistry, the principles of which rest rather on probable evidence than on demonstration, it is of importance to present a subject in every point of view under which it may be surveyed; and this must serve as an apology for the speculations I have now to offer.

There are, I believe, only two arguments to which any weight is due in support of the opinion that chlorine is a simple substance, which, by combination with hydrogen, forms muriatic acid. One is drawn from the analogy resting on the general fact, sufficiently established, that acidity is, in different cases, the result of the agency of hydrogen; the other, from the analogy in the chemical relations of chlorine and iodine.

Sulphur forms with hydrogen a compound unequivocally acid. The compound radical of prussic acid cyanogen, discovered by the able researches of Gay-Lussac, likewise acquires acidity when it receives hydrogen. Acidity, therefore, is a property not exclusively connected with oxygen; it is also communicated by hydrogen; and when chlorine with hydrogen gas forms muriatic acid gas, the agency exerted may be considered as similar to that arising in other cases of the production of an acid from the action of hydrogen.

This is confirmed by the relations of iodine. It too forms an acid by combination with hydrogen; and the chemical agencies of iodine are in several other respects similar to those of chlorine. When the one, therefore, is considered as a simple body (and there is no absolute proof that iodine is a compound), the other is, with probability, placed in the same class: and certain analogies existing between sulphur and iodine serve to connect and confirm these views. Each of them forms an acid with hydrogen; each of them also forms an acid with oxygen; but chlorine exhibits precisely the same points of resemblance: with hydrogen, it forms muriatic acid; with oxygen, it forms chloric acid. Its chemical relations, with regard to acidity, being thus similar, seem to require the same explanation to account for them.

These facts lead undoubtedly to views of chemical theory different from those which had before been established; and on which the old doctrine with regard to the nature of muriatic and oxymuriatic acid rests. It may be well, therefore, to inquire, how far they may modify the conclusions to be drawn, admitting even that oxymuriatic acid contains oxygen, and that muriatic acid gas affords water.

When water is obtained from muriatic acid gas, it does not necessarily follow that it has pre-existed in the state of water. It is equally possible, *à priori*, that its elements may be present in simultaneous combination with the acid, or its radical; that the acid is a ternary compound of a radical with oxygen and hydrogen; and that it is decomposed in those processes by which water is procured, the hydrogen, with the requisite proportion of oxygen, combining to form water; and its radical, with any excess of oxygen, remaining in union with the substance by which the change has been effected.

If this view were adopted with regard to muriatic acid, the same view might, on the same grounds, be applied to the other acids, which appear to contain water in intimate combination, and in a definite proportion. And such an acid, the radical and precise constitution of which are known, may be best adapted to illustrate the hypothesis.

Sulphuric acid affords water when it is submitted to the action of an alkaline base; and the quantity of this water appears to be definite, amounting to 18·5 in 100 of the strongest acid which can be procured in an insulated state; 100 parts of this acid, therefore, are considered as composed of 81·5 of real acid (consisting of 32·6 of sulphur and 48·9 of oxygen) with 18·5 of water. But if, instead of this view of its constitution, it be considered as a ternary compound of sulphur, oxygen, and hydrogen, its composition will be 32·6 of sulphur, 65·2 of oxygen, and 2·2 of hydrogen. In those processes by which water is obtained from it; in the action, for example, of an alkaline base, and subsequent exposure to heat, the composition is subverted by the affinities exerted; the hydrogen unites with the requisite proportion of oxygen, forming water, and the remaining oxygen with the sulphur unite with the base. In the action of a metal on the acid, there is the same result; only by the attraction of the metal to oxygen, the whole of that element is retained, and the hydrogen is disengaged.

Muriatic acid gas then, according to this doctrine, is the real acid, a ternary compound of a radical (at present unknown) with oxygen and hydrogen, exactly as sulphuric acid in its highest state of concentration is the real acid, a ternary compound of sulphur, oxygen, and hydrogen. When it is submitted to an alkaline base, the action exerted causes its decomposition; its hydrogen, and part of its oxygen, combine to form water; and its radical, with its remaining oxygen, unite with the base, forming a neutral compound, analogous to what other acids of a constitution form. When a similar result is obtained from action of a metal, its whole oxygen must be considered as retained, and its hydrogen is liberated.

Nitric acid in its highest state of concentration is not a definite compound of real acid with about a fourth of its weight of water, but a ternary compound of nitrogen, oxygen, and hydrogen. Phosphoric acid is a triple compound of phosphorus, oxygen, and hydrogen; and phosphorous acid is the proper binary compound of phosphorus and oxygen. The oxalic, tartaric, and other vegetable acids, are admitted to be ternary compounds of carbon, oxygen, and hydrogen; and are, therefore, in strict conformity to the doctrine now illustrated.

A relation of the elements of bodies to acidity is thus discovered different from what has hitherto been proposed. When a series of compounds exists, which have certain common characteristic properties, and when these compounds all contain a common element, we conclude with justice that these properties are derived more peculiarly from the action of this element. On this ground Lavoisier inferred, by an ample induction, that oxygen is a principle of acidity. Berthollet brought into view the conclusion that it is not exclusively so, from the examples of prussic acid and sulphuretted hydrogen. In the latter, acidity appeared to be produced by the action of hydrogen. The discovery by Gay-Lussac of the compound radical cyanogen, and its conversion into prussic acid by the addition of hydrogen, confirmed this conclusion; and the discovery of the relations of iodine still further established it. And now, if the preceding views are just, the system must be still further modified. While each of these conclusions is just to a certain extent, each of them requires to be limited in some of the cases to which they are applied; and while acidity is sometimes exclusively connected with oxygen, sometimes with hydrogen, the principle must also be admitted that it is more frequently the result of their combined operation.

There appears even sufficient reason to infer that from the united action of these elements, a higher degree of acidity is acquired than from the action of either alone. Sulphur affords a striking example of this. With hydrogen it forms a weak acid. With oxygen, it also forms an acid, which, though of superior energy, still does not display much power. With hydrogen and oxygen, it seems to receive the acidifying influence of both, and its acidity is proportionally exalted.

Nitrogen with hydrogen forms a compound altogether destitute of acidity, and possessed even of qualities the reverse. With oxygen in two definite proportions, it forms oxides; and it is doubtful, if in any proportion, it can establish with oxygen an insulated acid. But with oxygen and hydrogen in union, it forms nitric acid, a compound more permanent, and of energetic action.

Carbon with hydrogen forms compounds which retain inflammability without any acid quality; with oxygen, it forms first an inflammable oxide, and with a larger proportion a weak acid; but combined with both hydrogen and oxygen, in different proportions, it forms in the vegetable acids compounds having a high acidity. These acids, therefore, are not to be regarded, according to the theory of Lavoisier, as composed of a compound base of carbon and hydrogen, acidified by oxygen, but of a simple base, carbon, acidified by the joint action of oxygen and hydrogen.

Muriatic acid itself presents the same result. Oxymuriatic acid must be considered, according to this doctrine, as a compound of an unknown radical (*Murion*, if the term may be allowed) with oxygen, analogous in this respect to sulphurous acid, except that in the latter there is an excess of base, in the former an excess of oxygen: and oxymuriatic acid, with the addition of hydrogen, forms the ternary compound muriatic acid, as sulphurous acid with the same addition forms hydrosulphuric acid, with a deposition of the excess of sulphur. There is accordingly the strictest analogy between muriatic acid and those other acids, the sulphuric, nitric, &c. which contain both oxygen and hydrogen; while there is none, as Berzelius remarked, between it and those, such as the prussic acid or sulphuretted hydrogen, which contain merely hydrogen. This principle solves the difficulty which has always presented itself in the relation of muriatic and oxymuriatic acids on Lavoisier's theory of acidity—that the latter, though it has received an addition of oxygen, is inferior in acid power to the former. It is so precisely, as the binary sulphurous acid is one of less energy of action than the ternary hydrosulphuric acid, or as the carbonic is less powerful than the oxalic acid. The proper analogy is that of the oxymuriatic with the sulphurous acid, and the muriatic with the sulphuric; and under this point of view there is no anomaly, but strict conformity. And thus also is accounted for, what is at variance with the hypothesis of Gay-Lussac, the total want of analogy between chlorine and sulphur, which he classes together, except in the single circumstance of acidity being communicated to both by hydrogen; while there exists a close analogy between sulphurous acid and oxymuriatic acid in their most essential properties—their gaseous form, their specific gravity, their suffocating odour, their power of destroying vegetable colours, their solubility in water, their remaining combined with it in congelation, their acidity, their combining weights, and their being attracted to the positive pole of the voltaic series; and any deviation from this analogy evidently arises from the excess of oxygen in oxymuriatic acid.*

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* It is curious with regard to the most important of these analogies, that of the equivalent or combining weights, that oxymuriatic acid stands next to sulphurous acid; the former in Dr. Wollaston's scale being 44, while the latter will be found to be 40. The acidity of oxymuriatic acid is fully established by the most unequi-

It is obvious that it would be in vain to seek for the discovery of real muriatic acid in its insulated form. It exists no more than real sulphuric or real nitric acid. The oxygen and sulphur, or oxygen and nitrogen in union with a salifiable base in the sulphates and nitrates, may not be in direct combination, nor capable of existing as a separate binary compound. The insulated binary compound of the radical of muriatic acid with oxygen is oxymuriatic acid, as the binary compound of sulphur and oxygen is sulphurous acid, and of nitrogen and oxygen, nitrous and nitric oxides.

Iodine, the discovery of which and its relations has for a time given predominance to the new doctrine of chlorine, conforms sufficiently to these views. Some have considered it as a body belonging to the same class as chlorine; others regard it as more analogous to sulphur. It has little analogy to either, except in the property of forming acids with oxygen and with hydrogen. It differs remarkably from chlorine in its comparative inertness, its solidity, specific gravity, and great weight of its equivalent quantity; and it differs from sulphur in its want of inflammability, its solubility in water, and its being attracted to the positive pole of the voltaic series. All these analogies are preserved, and its relations connected, by considering it as an oxide, which, both from its specific gravity, the colour of its compounds, and the great weight of its equivalent quantity, has probably a metallic base; and which acquires acidity by an addition of hydrogen on the one hand, and on the other by the addition of oxygen, or of oxygen and hydrogen. In these respects, and in many of its chemical properties and relations, a considerable analogy exists between it and oxide of arsenic or oxide of tellurium. Or if it were to be classed as a simple substance (on the ground of its not having been decomposed), which forms an acid with hydrogen, and another with oxygen and hydrogen; it does not in these respects offer any deviation compared with other acidifiable bases, or afford an argument of much weight in support of the undecomposed nature of chlorine.

The doctrine I have illustrated affords a satisfactory explanation of the properties of the compounds formed by oxymuriatic acid with certain inflammables, particularly with sulphur and phosphorus. These undoubtedly present an anomaly in the other views that have been given of their constitution. In the old doctrine, they are considered as compounds of two real acids; one of muriatic, with phosphorous or phosphoric acid; the other of muriatic, with sulphurous or sulphuric acid. But

vocal acid property, that of combining with alkalies, and forming neutral compounds. The saline nature of these compounds had been shown by Berthollet; that with lime has been demonstrated by Mr. Dalton, who also pointed out the probability from the results by double decomposition, that the acid combines in a similar manner with other salifiable bases; and the existence of these compounds has been established by Mr. Wilson.

they have none of the properties which would be looked for in such a combination; they have no acidity, or if any appear in one of the compounds with phosphorus, it is to a very limited and doubtful extent; and they are substances even which have little energy of chemical action. In the new doctrine they are considered as compounds of chlorine with their bases, sulphur, and phosphorus. Of course, as these bases form powerful acids with oxygen, and as chlorine is considered as an element of similar agency as oxygen, communicating similar powers, and conferring acidity even on hydrogen, they might, with not less reason than on the other doctrine, be expected to be acids of the greatest strength. The view I have stated accounts for their characters. They are ternary compounds, of the radical of muriatic acid with the particular inflammable—sulphur, or phosphorus, with oxygen. The oxygen is not in sufficient quantity to communicate acidity, or, in one of the combinations of phosphorus, does so only to a very slight extent. But when water is added, a sufficient proportion of oxygen is supplied to produce this result, and the acidity is exalted by the corresponding hydrogen entering into the combination. What has been called phosgene gas, procured under certain circumstances from the action of oxymuriatic gas and carbonic oxide, may be regarded as of a similar nature, the agency of a small portion of water or of hydrogen being probably essential to its formation, a circumstance which serves to account for the discordant results with regard to its production.*

It deserves remark, that while there runs through the whole series of acidifiable bases in relation to their combinations with oxygen and hydrogen, a general analogy, there is also some deviation, and something with regard to each that is specific. Sulphur affords the most perfect example of their agency. It forms an acid with hydrogen; it forms another with oxygen; and a third, still more powerful, from the joint action of oxygen and hydrogen. Carbon forms an acid with oxygen; it also forms a series of acids of greater strength with oxygen and hydrogen; it acquires no acidity, however, from hydrogen alone; and with an inferior proportion of oxygen it forms an oxide. Phosphorus bears a strict analogy to sulphur, except that its combination with hydrogen does not give rise to acidity, a circumstance in which it resembles carbon. Nitrogen is peculiar in forming two oxides with different definite proportions of oxygen; it is doubtful if it forms a free acid with oxygen alone;

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* The difficulty of entirely excluding water and hydrogen from the constituents of this gas is sufficiently apparent. And the fact that it cannot be formed from them by the action of the electric spark, but only by the continued action of solar light, is favourable to the above opinion. The conversion of carbonic oxide into carbonic acid by the joint action of oxymuriatic gas and hy[illegible] experiment which I performed when the new hypothesis with regard to [illegible]e of chlorine was brought forward, and which was attempted to be inv[illegible]om controversial methods, I consider as depending probably [illegible] pr

but it conforms to the general law, and forms a powerful acid with oxygen and hydrogen. Assuming the existence of a simple radical of muriatic acid, it resembles sulphur, phosphorus, and carbon, in forming an acid with oxygen, and one still more powerful with oxygen and hydrogen; but it differs in the peculiarity, that the proportion of oxygen to the base in the binary combination is considerably larger than in the ternary, so that the addition of hydrogen converts the one into the other; and also in its combining apparently with more numerous proportions of oxygen than any of the other acidifiable bases, two circumstances which, as well as the difficulty of effecting its decomposition, probably depend on the same cause, the strength of its attraction to oxygen. The fluoric are similar to the muriatic compounds, except that the binary compound of the radical with oxygen cannot be obtained in an insulated form, and that its combinations with oxygen are less numerous. The relations of iodine or its radical are similar to those of the radical of muriatic acid, or perhaps rather to sulphur, except that its binary compound with oxygen does not appear to have acidity, in which it approaches to the metals. The metals usually combine with oxygen so as to form oxides; some of them also form acids with oxygen, or with oxygen and hydrogen; and these last usually also combine with hydrogen alone. This fact, of some of the metals forming acids, is so far an anomaly, since their compounds with oxygen rather form alkalies, and no other substances give rise to both results; the greater number of the substances too, which form acids with oxygen or hydrogen, are evidently, from the smallness of their combining quantities, not of a metallic nature. Still the connexion between the two classes is in some measure established on the one hand by nitrogen, which, with hydrogen, forms an alkali; and on the other by iodine, which has properties and relations common to both.

In some cases it is probable that there is a variation in the proportions of these ternary combinations, giving rise to a diversity of products, which exist only in combination with those bodies by which their formation is determined, and, being modified by any process causing their evolution, are not easily observed. It is doubtful if the same base in any case forms different acids by combination with oxygen in different proportions, or by combination with hydrogen in different proportions. But the example of the vegetable acids seems to show that this may occur in the united action of oxygen and hydrogen; carbon acidified by different proportions of these elements constituting the composition of these acids. Other bases may present similar results. The radical of muriatic acid may unite with other proportions of oxygen and hydrogen than those which form muriatic acid; and this might afford a solution of the theoretical difficulty of the production of water in the experiments in the first part of this memoir, independent of the explanation of it from the formation of a super-muriate. A compound may be formed with less oxygen and hydrogen than what exist in muriatic acid, in combination with the metal acted on, and thus a portion of water may be liberated. Nor will it be easy to establish this by any difference in the product, as it can scarcely be submitted to any examination, but by processes which change the result. The chloric acid which, according to Gay-Lussac, cannot exist insulated without water, may be in like manner a ternary compound of these elements in other proportions. Prosecuting the same analogy, the glacial, or fuming oil of vitriol may be, not what has lately been asserted, real sulphuric acid (for probably no such substance as that to which this term has been applied can be obtained insulated), but a compound of sulphur with oxygen and hydrogen, in proportions different from those which constitute common oil of vitriol. Nitrous acid, if it cannot be formed without water, may be a compound of nitrogen with a smaller proportion of oxygen and hydrogen than nitric acid. And some of the acids lately described, of which phosphorus is the base, may arise from variations of proportions of this kind.

The view which I have now illustrated, I must add, is not to be regarded as mere speculation. The evidence in support of it is just as conclusive as that from which the opposite opinion is inferred. The obtaining water from a compound is no necessary proof that water pre-existed in it; and conversely, the causing water to enter into combination in a compound is no necessary proof that it remains in the state of water in the product. In many cases we draw the reverse conclusions, considering water as being formed where it is obtained, and as decomposed where it is communicated. And in the case of its relation to acids, it will be found that there is no strict evidence of its existing as water in combination with what is considered as the real acid; and of course the conclusion is equally open to be drawn, that it exists in these combinations in the state of its elements, and that when obtained, it is a product of a change of composition.

It is even more probable, *à priori*, that the ultimate elements should act on each other where energetic affinities are evidently exerted, than the immediate principles, and the relations of these elements will determine the combinations and the proportions. And by admitting this view, we avoid the anomaly which is presented in ascribing to the agency of water effects so different from those to which it usually gives rise. In general, water operates on bodies simply as a solvent, overcoming cohesion in solids, diluting liquids, or absorbing gases, without otherwise modifying their properties, or communicating to them any important chemical powers. But in the particular ca now referred to, it is supposed to produce the effects of most energetic chemical agent; it enters into combinati proportions strictly definite; is retained by the most powerful affinities; communicates new and characteristic properties; and is essential even to the existence of these compounds in an insulated form. Berzelius and Gay-Lussac have stated, that it is to be considered as a base necessary to retain the elements of the acid combined, though without neutralizing the acid properties—an opinion which in itself, and still more with this condition, is certainly sufficiently incongruous. And both theories admit equally of incongruity in the supposed presence and energetic action of water in acids. The old doctrine admits its influence in sulphuric, nitric, phosphoric, and muriatic acids, though at variance with its principle, that oxygen is the element which confers acidity, or at least having no conformity to that principle, nor receiving explanation from it. The new doctrine refuses to admit it with regard to muriatic acid, but admits it in all the others—an exception which serves only to render the system more objectionable by the violation of analogy; while the admission with regard to the others is equally incapable of being accounted for on any principle it affords. By considering oxygen and hydrogen as elements conferring acidity, a satisfactory solution is afforded of the effects produced in these cases by their joint operation; and independent of this, it is much more probable, *à priori*, that such effects should arise from the action of elements so powerful, than from the agency of water, which, in its general relations, exerts such feeble powers. Lastly, the principle on which the presence of combined water in these acids has been supposed to depend, that of the strong attraction of the acid to water, seems altogether fallacious; for on this principle sulphurous acid should also contain combined water and sulphuretted hydrogen, and even carbonic acid might be expected to retain a small portion. The whole evidently depends on difference of constitution. Sulphurous acid, sulphuretted hydrogen, and carbonic acid, are binary compounds, and therefore yield no water, nor retain any in intimate combination; and in the others, the proportion of water supposed to exist will be found to have no relation to the attraction of the acid to water, so far as this can be inferred, as is evident from the example of phosphoric acid affording as much as sulphuric or nitric; but to the relations of its elements, and more particularly of its oxygen, to the radical. This last fact affords nearly a demonstration that the constitution is that of simultaneous combination of the elements, and not that of water and acid.

That water may also exist in immediate combination with acids, without being resolved into its elements, is sufficiently possible; and it probably is in this state in those cases in which there are no indications of an intimate combination, or definite proportion. It may then be considered as in solution similar to that in which it holds salts dissolved, or, what is a closer analogy, similar to that in which it holds dissolved the vegetable acids, which are admitted to be ternary compounds of carbon, hydrogen, and oxygen. The opposite view applies only to that portion of water considered as essential to the body in an insulated state, and in which it is combined in a definite proportion, observing in its relations, or the relations of its elements, equivalent proportions to other bodies.

In the last place, considering this opinion in relation to the two opposite views which have been maintained with regard to the constitution of oxymuriatic and muriatic acids, while it has all the evidence in its favour from which the existence of water in muriatic acid gas is inferred, and all the analogies by which this is confirmed; it has the support which the doctrine of the undecompounded nature of chlorine derives from the relations of sulphur, iodine, and cyanogen; and from the induction that hydrogen, as well as oxygen, communicates acidity. It avoids, at the same time, the improbability which attends that doctrine, in its leading principle, that muriatic acid contains no combined water, though other powerful acids are held to contain it, and though it affords water by the very same processes by which they yield it; and in the still greater violation of analogy (the most extraordinary perhaps ever admitted in chemical reasoning), involved in the conclusion that the compounds which this acid forms with salifiable bases, though the same in all generic properties with those formed by other acids, are not of similar constitution, and are not even of a saline nature. It unites the advantages, therefore, of both doctrines, and connects, under one system, facts which are otherwise insulated, and partial generalisations, which, instead of having any relation, seem opposed to each other.

The same general view, I have still to add, may be further extended. Alkalinity, as well as acidity, is the result apparently of the action of oxygen; the fixed alkalies, the earths, and the metallic oxides, which all contain it as a common element, forming a series in which it is difficult to draw any well defined line of distinction. Ammonia alone remains an exception: it contains no oxygen, and yet possesses in a very marked degree all the alkaline properties—an anomaly so great, as to have led almost every chemist to infer that oxygen must exist as an element in one or other of its constituent principles; and as nitrogen is the one apparently least elementary, it has been supposed to be a compound containing oxygen. The result may be accounted for, however, on a very different principle. As hydrogen, in some cases, gives rise, as well as oxygen does, to acidity, so it may, in other cases, give rise to alkalinity. Under this point of view, ammonia is a compound of which nitrogen is the base, deriving its alkaline power from hydrogen; it stands, therefore, in the same [illegible] to the other alkalies that sulphuretted hydrogen does to [illegible]ls. [illegible] the whole speculation with regard to the imaginary metallic base ammonium, and the existence of oxygen in ammonia and in nitrogen falls to the ground, while the anomaly presented by this alkali is removed. If the claim of the lately discovered principle in opium, *Morphia* as it has been named, to the distinction of an alkali be established, as from its origin it must probably have a compound base, it may, if it contain hydrogen, bear the same relation to the other alkalies that prussic acid does to the acids; or if it contain oxygen, it will be analogous to the vegetable acids.

The fixed alkalies and the alkaline earths are considered as containing water in intimate combination in a definite proportion; and it is doubtful if they can be obtained free from it in an insulated state, retaining at the same time their alkaline properties. It is obvious, however, that the elements of water may exist in combination with the base; that potash, for example, is not a compound of an oxide of potassium with water, but of potassium, oxygen, and hydrogen. Hence when, on adding water to peroxide of potassium, potash is produced, and oxygen gas is disengaged; this is not owing, as has been supposed, to the excess of oxygen in the peroxide being expelled, and the water taking its place; but to the water being decomposed, and a portion of its hydrogen entering into the combination, to form the alkali, while the corresponding oxygen is liberated. If hydrogen were brought to act on peroxide of potassium, the alkali would in like manner be formed. With the peroxide of barium, this very change, from the action of hydrogen, takes place; the hydrogen, according to the usual explanation, combining with its oxygen, and forming water, which unites with the real earth, forming the hydrate; in other words, and according to the strict expression of the fact, the hydrogen entering into the composition, and forming the barytes; a result perfectly analogous to the formation of muriatic acid from oxymuriatic gas by the agency of hydrogen.

The evidence in support of this doctrine, it is evident, is of the same kind as that with regard to the doctrine applied to the acids. There is the same superior probability in favour of the conclusion that the elements of water rather than water itself exist in these compounds, from the consideration that modifications of properties so important are more likely to arise from the agency of these elements than from any action which water can exert. And that water does not exist in them in consequence of the strength of attraction which the real alkali, as it has been considered, exerts towards it, is evident from this, that on the same principle ammonia ought to contain combined water in its insulated form, which is not the case. The combination of water, therefore, or rather of its principles, in these compounds, depends on relations subsisting among the ultimate elements, not on an affinity exerted by the alkali itself; and this adds confirmation to the conclusion, that these elements are in ternary union.

Their superior alkaline energy compared with the common metallic oxides may obviously arise from the joint action of the hydrogen and oxygen, in the same manner that the acidity of the ternary compared with the binary acids is increased by a similar constitution. Thus the class of alkalies will exhibit the same relations as the class of acids. Some are compounds of a base with oxygen: such are the greater number of the metallic oxides, and several, probably, of the earths. Ammonia is a compound of a base with hydrogen. Potash, soda, barytes, strontites, and, probably, lime, are compounds of bases with oxygen and hydrogen; and these last, like the analogous order among the acids, possess the highest power. Many of the metallic oxides, however, in the state in which they combine with the greatest facility with the acids, are hydrates; that is, supposed compounds of the oxide with water, but probably ternary compounds of the metal with oxygen and hydrogen; and their facility of combination may depend on this constitution. The same principle explains the necessity, not otherwise easily accounted for, of the presence of water, to enable some of the earths, as barytes, to combine with acids.

There are two views under which the neutral salts may be considered in the preceding theory. It has been shown, that when water is obtained in the action of a salifiable base, whether alkali, earth, or metallic oxide, there is reason to infer that this water is formed by the hydrogen and part of the oxygen of the acid entering into binary combinations; and when water is obtained from an alkali by the action of an acid, there is the same reason to believe that it is formed by the combination of the hydrogen of the alkali with a portion of its oxygen. In these cases it may be supposed, that the radical of the acid combines with its remaining oxygen, forming a binary compound, which may still be considered as an acid; and that the radical of the alkali combines with its remaining oxygen, forming a binary compound, which may be regarded as an alkali; and these two compounds may unite with each other, forming the neutral salt. This is conformable nearly to the common doctrine. But there is another point of view under which the subject may also be considered. A ternary combination, into which oxygen and hydrogen enter, gives rise apparently to a higher state of acidity, and to a greater degree of alkaline energy than is acquired from a mere binary combination into which oxygen enters. It is doubtful, therefore, if such binary compounds were formed, if they would constitute either acid or alkali. And there is at least no proof of their formation. In all these cases, while the hydrogen r[illegible] combines with the requisite proportion of oxygen formi[illegible]he radical of the acid and the radical of the base m[illegible] the remaining oxygen, and form a ternary compound. And where hydrogen is not present, such a combination may be at once established.

It is not easy to determine which of these opinions is just. The reason above stated renders the latter, perhaps, more probable; and the view which leads to the conclusion, that in the constitution of the acids and alkalies the three elements, when present, are in simultaneous combination, leads also to a similar conclusion with regard to the constitution of the neutral salts. If this be adopted, neutralization is not the saturation of acid with alkali, and the subversion of the properties of the one by the opposed action of those of the other, but is the change of composition of both, and the quiescence of the elements in that proportion in which their affinities are in a state of equilibrium without any excess. The compounds, therefore, have little activity; and energy of action is restored only by the reproduction of substances, which, by their mutual attractions, tend to the same state of quiescence.

All these results display more fully the extensive relations of the two elements, oxygen and hydrogen. They do not act merely in opposition, as had been imagined, but more frequently in union, producing similar effects. Hydrogen is of nearly equal importance with oxygen; and the principal details of chemistry consist in their modified action on inflammable and metallic bodies.

Article VI.

On the Persulphates of Iron. By Mr. Cooper.

(To Dr. Thomson.)

DEAR SIR, 89, *Strand, March* 16, 1819.

I feel very happy in being able to confirm your analysis of the persulphates of iron contained in Vol. X, No. LVI, of your *Annals of Philosophy*, and also to verify the conjecture you have thrown out of there being a persalt of iron containing an excess of sulphuric acid. This salt I have formed by boiling recently precipitated peroxide of iron (from nitric acid by ammonia) in a considerable excess of sulphuric acid; the solution goes on but slowly; but when obtained, if it be evaporated to the consistence of syrup, it will in a few days deposit crystals: these are the bipersulphate of iron. I find the crystallization to succeed better when a small excess of acid is present. The form of the crystal is that of an octohedron; some of the solid angles are truncated, some of the edges are bevelled, and in others the edges are truncated, while others of them are the perfect octohedron. These crystals are permanent, and are perfectly transparent and colourless; they do not at all indicate the presence of iron by the taste, which exactly resembles that of alum. They are very soluble in water, and contain a considerable quantity of that fluid combined, as will be shown in the sequel. They undergo the watery fusion; and, when fused in their own water of crystallization, immediately change their colour and become red; and if the whole of the water be driven off by heat, the dry mass is converted into two substances, one of which is soluble, but the other and greater portion insoluble in water: this latter, however, is readily taken up by the addition of muriatic acid. Three hundred and eighty grains of it were dried at a temperature of about 300° Fahr. and lost 200 gr. of water: the whole, being redissolved by the addition of a small quantity of muriatic acid, the oxide of iron precipitated by ammonia, and the sulphuric acid by muriate of barytes, gave of

Peroxide of iron.	60 gr.
Sulphate of barytes 352 gr. = sulphuric acid. .	120
Water	200
	380

This is the mean of three experiments; and from these data, you will perceive the composition of this salt to be

1 atom peroxide of iron
+ 2 atoms sulphuric acid
+ 15 atoms of water, or

1 atom of peroxide of iron.	40
2 atoms sulphuric acid	80
15 atoms water.	135

considering hydrogen as unity, and oxygen 8.

I feel confident of the existence of another salt of iron containing a still larger quantity of acid; and I have little doubt that I shall be able to obtain it in a distinct form. If I should be successful, I shall send you the results; but hitherto I have obtained it only in very minute crystals, and which it is difficult to free from the adhering excess of acid. The way in which I have formed this salt is as follows:—After separating two crops of crystals of the bipersulphate, I added sulphuric acid to the mother liquor, and evaporated till a pellicle formed on the surface: on cooling, the salt in question separated. These crystals appear, when very highly magnified, in the form of quadrangular plates. They are slightly deliquescent (but this probably may be owing to the adhering acid); they are perfectly white, and have a pearly lustre; their taste is very acid, but not so astringent as the former salt; when caustic alkalie [illegible] ed to them, they immediately indicate the presence o[illegible] of iron. Owing to the above-mentioned circumstance [illegible] not being able to get this substance in a form fit, I [illegible] have not yet attempted it; but have little doubt of its being the salt you predicted, composed of one atom oxide of iron, and three atoms sulphuric acid.

I mean to continue the investigation of these singular salts; and if I am so fortunate as to discover any more of their properties, I will transmit you the results. I remain, dear Sir,

Very sincerely yours,

John Thomas Cooper.

Article VII.

Analyses of Books.

Philosophical Transactions of the Royal Society of London, for 1818, *Part II.*

(*Continued from p.* 218.)

VI. *Observations on the Heights of Mountains in the North of England.* By Thomas Greatorex, Esq. F.L.S.—Mr. Greatorex, during the summer of 1817, measured the height of Skiddaw above Derwentwater by levelling. At the same time, he observed the height by an excellent mountain barometer, made by Ramsden, while Mr. Crosthwaite, of the Keswick Museum, observed another barometer, placed 10 yards above Derwentwater, every half hour. An observation was made by means of the mountain barometer during the levelling at every 50 yards of descent. The following are the results obtained by these different measurements:

	Yds.	Ft.	In.
Skiddaw, above the lake.	936	0	3½
Derwentwater, above the sea at low water-mark by Mr. Crosthwaite's measurement	76	0	0
Height of Skiddaw above the sea by levelling	1012	0	3½

	Inches.	
Barometer below (10 yards above the lake)	30·050	therm. 61°
Barometer above.	27·156	attached therm. 57°; detached ditto 50°.

	Yards.		Error.
Hence the height by Dr. Maskelyne's formula.	926·1685	..	+ 0·1685
By Dr. Hutton's formula........	925·2850	..	− 0·7150

At 50 yards down,

Height by measurement	876·000000	..	—
By Maskelyne's formula........	873·194477	..	− 2·805523
By Hutton's formula.	—	..	− 3·948000

	Yards.	Error.
At 100 yards down,		
Height by measurement	826·00000	—
By Maskelyne's formula	821·71247	− 4·28753
By Hutton's formula.	—	− 4·60320
At 150 yards down,		
Height by measurement	776·0000	—
By Maskelyne's formula	772·3057	− 3·6943
By Hutton's formula.	—	− 4·6444
At 200 yards down,		
Height by measurement	726·0000	—
By Maskelyne's formula	724·7245	− 1·2755
By Hutton's formula.	—	− 2·0920
At 250 yards down,		
Height by measurement	676·000	—
By Maskelyne's formula	672·252	− 3·7480
By Hutton's formula.	—	− 4·3152
At 300 yards down,		
Height by measurement	626·000	—
By Maskelyne's formula	616·731	− 9·2690
By Hutton's formula.	—	− 9·6532
At 350 yards down,		
Height by measurement	576·0000	—
By Maskelyne's formula	566·3645	− 9·6355
By Hutton's formula.	—	− 9·9100

Dist. from summit.	Height by measurement.	Error of barometrical measurement.	
		By Maskelyne's formula.	By Hutton's formula.
Yards.	Yards.	Yards.	Yards.
400	526	−12·687200	−12·8400
400*	526	− 8·102470	− 8·6740
450	476	− 5·733300	− 6·2514
500	426	− 8·116700	− 8·6500
550	376	−10·157300	−10·6186
600	326	− 2·901242	− 3·3260
650	276	− 9·108500	− 9·4460
700	226	− 4·155800	− 4·4460
750	176	− 4·667000	− 4·8880
800	126	− 8·180000	− 8·2996
850	76	− 6·631820	− 6·4918
900	26	00	+ 3·3130

* By another

Dr. Maskelyne's formula for determining the height of mountains by the barometer is as follows:

1. Take the difference of the tabular logarithms of the observed barometrical heights at the two stations, considering the first four figures (exclusive of the index) as whole numbers.

2. Observe the difference of Fahrenheit's thermometer at the two stations; multiply this difference by 0·454, and add or subtract this product, according as the thermometer was highest at the upper or low station, which will give an approximate height.

3. Take the mean between the two altitudes of the thermometer, and find the difference between this mean and 32°. Multiply the approximate height by this difference, and the product by the decimal fraction 0·00244. This last correction being added to, or subtracted from the approximate height, according as the mean of the two altitudes of Fahrenheit's thermometer was greater or less than 32°, will give the true height of the upper station in English fathoms.

Dr. Hutton's rules are as follows:

1. Let the heights of the barometer at the top and bottom of any elevation be observed as near the same time as may be, as also the temperatures of the attached thermometers, and the temperature of the air in the shade at both stations, by means of detached thermometers.

2. Reduce these altitudes of the barometer to the same temperature by augmenting the height of the mercury in the colder temperature, or diminishing that in the warmer by its $\frac{1}{9600}$ part for every degree of difference between the two.

3. Take the difference of the common logarithms of the two heights of the barometer (so corrected), considering the first four figures as whole numbers, which will give an approximate height.

4. Take the mean of the two detached thermometers: and for every degree which this differs from 31°, take so many times the $\frac{1}{435}$ part of the approximate height; and *add* them, if the mean temperature be above 31°; but *subtract* them if it be below 31°; and the sum or difference will be the true altitude in fathoms.

These formulas have been somewhat modified, and, perhaps, improved by subsequent philosophers. But the improvements would not remove the striking anomalies observable in the preceding table from Mr. Greatorex's observations.

VII. *On the different Methods of constructing a Catalogue of the fixed Stars.* By J. Pond, Esq. F.R.S. Astronomer Royal.—This paper contains a set of judicious observations highly worthy of the attention of astronomers. But as it would be in vain to expect that a subject of such a nature would be interesting to readers in general, we shall not attempt an abstract of it. We learn a curious fact from this paper, which redounds highly to the credit of the instruments at the Greenwich Observatory and of the Astronomers Royal. The late Dr. Maskelyne had constructed a catalogue of the fixed stars, and Mr. Pond has done the same in quite a different way. Yet the position of the stars in the two catalogues coincides within a small fraction of a second.

VIII. *A Description of the Teeth of the Delphinus Gangeticus.* By Sir Everard Home, Bart. V.P.R.S.—The Delphinus gangeticus was described by the late Dr. Roxburgh in the seventh volume of the Asiatic Researches, published in 1781. It is noticed by Dr. Shaw in the second volume of his General Zoology, published in 1801; but so inaccurately, that the description seems rather to apply to another animal. The author of this paper got a specimen of the upper and lower jaw of this animal from Sir Joseph Banks 17 years ago, which has been deposited ever since in the Hunterian collection. But it was only the other day that he discovered by an accidental reference to the Asiatic Researches the name of the animal to which the jaws belonged. He gives a figure of these jaws, and a short description of the teeth. They are 120 in number, 30 in each jaw. The upper part of the tooth, which is covered with enamel, has the figure of the point of a flattened cone. The under part is destitute of enamel, spreads out, increasing considerably, in breadth, but not in thickness, till it is at last imbedded in the substance of the jaw itself.

IX. *Description of an acid Principle prepared from the Lithic or Uric Acid.* By William Prout, M.D.—The author has ascertained that the beautiful pink substance formed when uric acid is heated with nitric acid is a compound of a peculiar acid, to which Dr. Wollaston gave the name of *purpuric*, and ammonia. It may be formed by dissolving uric acid in dilute nitric acid. The excess of nitric acid is then to be saturated with ammonia, and the whole slowly concentrated by evaporation. As the evaporation proceeds, the colour of the liquid becomes a deeper purple, and dark red granular crystals soon begin to separate in abundance. These crystals are dissolved in caustic potash, and heat applied till the red colour entirely disappears. The alkaline solution is then gradually dropped into dilute sulphuric acid, which uniting with the potash, the acid is deposited in a state of purity. Uric acid is likewise converted into purpurate of ammonia by chlorine and iodine.

Purpuric acid has a slightly yellow or cream colour. It has no smell or taste. Its specific gravity is considerably higher than that of water. It is scarcely soluble in water; that liquid not being capable of dissolving $\frac{1}{10000}$th of its weight of purpuric acid. It is insoluble in alcohol and ether. It dissolves in the concentrated mineral acids, and in the alkaline solutions it dissolves readily. But it is insoluble in dilute sulphuric, muriatic, and phosphoric acids, and likewise in oxalic, citric, and tartaric acids. Nitric acid dissolves it with effervescence, and converts it into purpurate of ammonia. It does not attract moisture from the air, nor redden litmus paper. When kept, it acquires a red colour, and is partly converted into purpurate of ammonia.

Its constituents, according to the analysis of Dr. Prout, are as follows:

Hydrogen 2 atoms	= 0·25	or per cent.	4·54
Carbon 2 atoms	1·50		27·27
Oxygen 2 atoms	2·00		36·36
Azote 1 atom	1·75		31·81
	5·50		99·98

But as his analysis was limited by the small quantity of acid in his possession, he seems to place but little confidence in the accuracy of the preceding numbers.

The *purpurate of ammonia* crystallizes in four-sided prisms, which, when viewed by transmitted light, have a deep garnet red colour. But by reflected light, the two broadest faces appear of a brilliant green, while the other two faces appear of dull reddish brown colour. It is soluble in about 1500 parts of water, and the solution has a deep carmine red colour. The solution has a slightly sweetish taste; but no smell.

The other purpurates were obtained by double decomposition from purpurate of ammonia.

Purpurates of potash and soda are red. They may be obtained in crystals, which resemble in colour purpurate of ammonia. The latter is much more insoluble in water than the former.

Purpurates of lime, strontian, barytes, are green-coloured powders, which form reddish purple solutions in boiling water.

Purpurate of magnesia is a very soluble salt of a most beautiful purple.

Purpurate of ammonia does not throw down gold, platinum, copper, lead, nickel, or iron, from their solutions in acids.

Silver is thrown down deep purple; mercury, reddish purple; zinc, golden yellow; tin, pearl white; and cobalt, reddish.

X. *Astronomical Observations and Experiments, selected for the Purpose of ascertaining the relative Distances of Clusters of Stars, and of investigating how far the Power of our Telescopes may be expected to reach into Space when directed to ambiguous celestial Objects.* By Sir William Herschel, Knt. Guelp. LL.D. F.R.S.—As it would be scarcely possible to render this curious but rather intricate subject intelligible to our readers without devoting to it a greater portion of space than we can well spare, I am under the necessity of referring those who wish to study the subject to the paper itself.

XI. *On the Structure of the poisonous Fangs of Serpents.* By Thomas Smith, Esq. F.R.S.—When the poisonous fangs of serpents are attentively examined, a slit, or sutor, may be observed extending along the convex side from the foramen at the base to the aperture near the point. This is the consequence of the mode of formation of the tube which Mr. Smith has first pointed out. The tube he has found to be completely external, and formed by a deep longitudinal depression in the surface of the pulp, which is destined to become the tooth. When the pulp is converted into tooth, the edges of it come gradually into contact, and thus convert the depression into a tube. This tube is not lined with enamel, and in the common viper to the two sides of the tooth are cemented together by the enamel, which thus constitute the sutor of the tooth.

XII. *On the Parallax of α Aquilæ.* By John Pond, Esq. F.R.S. Astronomer Royal.—From a set of observations made with the telescope, erected for the express purpose of observing this star, in which it was compared with *l* Pegasi, Mr. Pond considers himself entitled to conclude, that it exhibits no evidence whatever of having a parallax.

XIII. *On the Parallax of the fixed Stars in right Ascension.* By John Pond, Esq. F.R.S. Astronomer Royal.—The observations contained in this paper coincide with those formerly made by Mr. Pond, in showing that the parallax of the brightest stars cannot possibly exceed half a second, and that it is very unlikely that it should amount to half that quantity.

XIV. *An Abstract of the Results deduced from the Measurement of an Arc of the Meridian, extending from Lat.* 8° 9′ 38·4″ *to Lat.* 18° 3′ 23·6″ *N. being an Amplitude* 9° 53′ 45·2″. By Lieut.-Col. William Lambton, F.R.S. 33d regiment of foot.—Some account of the results of this interesting measurement was given in the last number of the *Annals of Philosophy*. Col. Lambton is in hopes that the measurement of the arc will be continued still further north, and that at some future period it may be extended even as far north as Delhi.

Article VIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Feb. 25.—A paper, by Sir H. Davy, was read, on the formation of mists in particular situations. The author commenced by observing, that the fall of temperature after sun-set is greater on land than on water; and referred to the well-known peculiarity in the expansibility of water at temperatures below 40°, as the cause by which both the water and the superincumbent air are preserved at a superior temperature. When, therefore, according to Sir H. Davy, the cold and comparatively dry land air mixes with the warmer and moister air resting upon the water, the dis [illegible] of the temperature of the latter occasioned by thi [illegible] ey to separate a portion of its moisture i [illegible]

At this meeting also, a paper, by Capt. E. Sabine, was read, entitled "Observations on the Dip and Variation of the Magnetic Needle, and on the Intensity of the Magnetic Force, made during the late Voyage in Search of a North-West Passage." The author stated, that the dipping needle employed in these observations was similar to that described by Mr. Cavendish, and made by the same artist. It was so adjusted, that on reversing the poles, the dip remained unaltered; and it was placed in the direction of the magnetic meridian by a compass, placed at such a distance as to remain during the observation for the purpose of occasional verification.

In determining the intensity of the magnetic force, a magnet was employed to draw the needle to a horizontal position. The magnet was then removed at an observed moment, and the needle permitted to oscillate till the arcs became too small to be observed. At every tenth vibration both the arc and time were noted.

The azimuth compasses employed by Capt. Sabine to determine the magnetic variation, were made upon Capt. Kater's improved plan. The observations were generally made upon the ice, to avoid the great irregularities produced on board by the iron of the ship. The results of the whole of these different classes of observations were arranged in the form of tables.

March 4.—A paper, by Dr. Brewster, was read, on the action of crystallized surfaces upon light. Malus had remarked that the action exerted upon light by the first surface of iceland spar is independent of the position of its principal section; that its reflecting power extends beyond the limits of the polarising forces of the crystal; and that as light is only polarised by penetrating the surface, the forces which produce extraordinary refraction begin to act only at this limit. He also remarked, that the angle of incidence at which this spar polarises light by partial reflection is 56½°; and that whatever be the angle included between the plane of incidence and the principal section of the crystal, the ray reflected by the first surface is always polarised in the same manner. After stating these observations of Malus, Dr. Brewster proceeded to observe, that his experiments upon the subject led him to draw different conclusions, and rather seemed to indicate that the polarising forces extend beyond the crystal. He also showed that the force of double refraction and polarisation originate from the surface of bodies, though its intensity depends upon the inclination of the surface to the axis of the crystal, and that the ordinary and extraordinary image may be extinguished at pleasure, and thus a doubly refracting crystal be converted into a singly refracting one. He also showed that the change in the angle of polarisation produced by the interior force depends on the inclination of the reflecting surface to the axis of the crystal and upon the azimuthal angle which the plane of reflection forms with the principal section; and that the change in the direction of the polarisation depends upon the angle which the incident ray forms with the axis of the crystal. The paper contained numerous experimental details.

At this meeting there was also read a paper, by Sir E. Home, giving an account of the fossil skeleton of an animal, several parts of which have been already laid before the Society in three separate papers. The author, after referring to his former papers, proceeded to describe, in general terms, and principally with the view of correcting his previous account, a specimen recently found nearly in an entire state. The only parts wanting were some of the bones of the pelvis and the lower part of the sternum. A beautiful drawing of the animal of its natural size accompanied the paper, which rendered minute description unnecessary.

March 11.—A paper, by C. Bonnycastle, Esq. was read, entitled, "On the Pressures which sustain a heavy Body in Equilibrium when the Points of Support are more than three." The author, after some general remarks, observed, that this is a problem which has hitherto never been satisfactorily investigated, though its assistance is necessary in estimating the strength of bridges and materials in general, and in determining the deflection and curvature of elastic plates. The difficulties attending the investigation of this problem were referred by Mr. B. to the too great generality of the method of investigation hitherto employed, and which, for the most part, has consisted not in the direct solution of the equation, but in comparing it with another admitting of a more easy solution. The author, after some further remarks, observed, that there is no method purely mathematical by which the difficulty can be surmounted; and that when abstractedly considered it appears impossible to determine the pressures which a heavy body exerts when supported on four or more fulcra. By considering, however, the circumstances under which pressure is usually generated, we shall be enabled, the author continued, to discover the law of its distribution, and this law must always govern its proportional quantities and intensities. Mr. B. then proceeded to examine the subject in this point of view, and to explain the general law of the distribution of pressure on determinate fulcra in different instances. The paper concluded with an investigation of the case, when the number of points of support is infinite; or in other words, consists of a line or plane surface.

March 18.—A letter by Dr. Granville was read, the object of which was to correct a mistake in his paper in the last volume of the Transactions of the Royal Society, and which had been pointed out to him by Dr. Maton.

SOCIETY FOR THE ENCOURAGEMENT OF ARTS, MANUFACTURES, AND COMMERCE.

This Society has voted the following rewards since February last:

Mr. Barraud's Double Violincello.—This i[illegible]nt possesses a greater compass than the double bass, and can be performed upon with as much facility as the violincello; and as its tone is much less powerful than that of the double bass, it will be found a most desirable acquisition in chamber concerts. The Society awarded its gold Isis medal to Mr. Barraud for this invention.

Mr. G. Rhodes's Warp Drier.—The warp is wound upon a sort of reel in a spiral manner, leaving spaces between each coil for the air to enter and dry the warp: it will be found very useful to woollen weavers, as it enables them to dry their warps in the house instead of exposing them at full length along the highways, &c. as usual. The Society voted its silver Isis medal and 10 guineas to Mr. Rhodes for this invention.

Mr. Donovan's British Cured Herrings.—Mr. Donovan having resided for a long time in Holland, and witnessed the methods used there in curing their celebrated herrings, was at the pains of repeatedly bringing over Dutch fishermen and curers to instruct the Highland fishermen in those processes, and has completely succeeded therein. The Society has awarded its premium of the gold medal, or 50 guineas, to Mr. Donovan for this laudable undertaking.

Mr. Thomas Taylor's Repeating Alarum.—This instrument has been found of great service in the Royal Observatory at Greenwich, in giving the astronomer timely warning of the passage of certain stars, &c. either by day or night, and thereby affording him opportunities of making observations which might otherwise be lost. The Society voted the sum of 15 guineas to Mr. Taylor for this invention.

Mr. Richard Green's Gauge, or Plough, for cutting Leather Straps, &c.—This instrument will be found useful to saddlers, bridle cutters, harness makers, &c. as it performs its work with great accuracy and expedition, and the edge of the knife is never injured by coming into contact with the cutting board. The Society awarded its silver Isis medal to Mr. Green for this invention.

Mr. William Feetham's Chimney Sweeping Apparatus.—By the introduction of a door into the flue, as near the top of the chimney as convenient, with a pulley affixed to it, that part of the chimney below the door may be conveniently swept by means of a line and brush, with an iron ball in a swivel affixed thereto; and the part above the door may be cleaned by another brush attached to a flexible handle; and in most cases without the use of climbing boys. The Society adjudged its silver medal to Mr. Feetham for this invention.

Mr. Fayrer's Clock.—This is an improvement on the three-wheeled clocks recommended by Dr. Franklin and Mr. Ferguson, by which they will go longer without winding up, and will continue in action whilst winding. The Society voted its silver Isis medal to Mr. Fayrer for this invention.

Mr. William Bullock's Screen Spring.—The object of this invention is to do away the injury which the pillars of screens sustain from the rubbing of the springs in common use; it is also applicable to other useful purposes. The Society awarded its silver Isis medal and five guineas to Mr. Bullock for this invention.

ARTICLE IX.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Carbonate of Morphia.*

In the number of the *Annals of Philosophy* for February, page 155, I gave the analysis of carbonate of morphia by M. Choulant. The proportions in which the morphia and carbonic acid are united, according to that chemist, differ so much from the proportions in which morphia unites with other acids, according to the same person, that there must obviously be an error either in the one determination or the other.

Sulphate of morphia is, according to Choulant, a compound of

Acid	5
Morphia	9

Here 9 represents the weight of an atom of morphia. By the acetate, it weighs 7·791; by the tartrate, 7·178; and by the nitrate, its weight is 12·15; but by the carbonate, it weighs only 2·16.

I shall here state the method by means of which Choulant formed the carbonate of morphia in question, and his mode of analysis, to enable my readers to determine the degree of confidence which they may put in the results which he has given us.

He put a drachm of pure crystallized morphia in four ounces of distilled water, and forced carbonic acid into the liquid by pressure till the morphia dissolved. A mixture of eight parts dry muriate of lime and six parts snow was then placed round the cylindrical glass vessel containing the liquid. The carbonate of morphia was precipitated in crystals, and obtained pure by throwing the whole upon a filter.

To analyze the salt thus formed, he put 100 gr. of it into a small retort connected with a Woulfe's bottle, containing barytes water. By the application of a moderate heat, the carbonic acid separated, and threw down the barytes in the state of carbonate: 22 gr. of pure morphia remained in the retort. The carbonate of barytes formed weighed 130·8 gr. This he considers equivalent to 28 gr. of carbonic acid. In [illegible] it is equivalent to about 28·8 gr.

He dissolved another 100 gr. of carbonate of mo[illegible]

and added acetate of lead as long as carbonate of [illegible].

to fall. The precipitate weighed 175·2 gr. This he considers equivalent to 28 gr. of carbonic acid. It is in reality equivalent to 28·7 gr. Thus these two analyses corroborate each other.—(See Gilbert's Annalen, vol. xlix. 412.)

II. *Iodine.*

Lampadius has observed that iodine dissolves with great facility in sulphuret of carbon, and gives it a dark reddish brown colour. One grain of iodine gives a deep colour to 1000 gr. of this liquid. Hence he recommends the sulphuret of carbon as an excellent reagent for detecting the presence of iodine.

III. *Analysis of the Tourmalin.*

I mentioned in the number of the *Annals of Philosophy* for last July, that boracic acid had been detected as a constituent of the tourmalin. This discovery was announced by Lampadius in March, 1818. It was made, he says, in his laboratory, by Mr. Breithaupt and himself. Since that time, the tourmalin has been examined by Dr. Gmelin, of Tubingen, with the express object of verifying the discovery of Lampadius. But hitherto his efforts to detect the presence of boracic acid have been unsuccessful. He had repeatedly analysed the tourmalin in Berzelius's laboratory, and had always met with the same loss as had occurred to Bucholz.

IV, *Phosphate of Iron.*

M. Vogel, of the Academy of Sciences at Munich, has made a set of experiments to determine the composition of the different phosphates of iron. Native prussian blue (as it is called) he found composed of

Water	31·0	
Protoxide of iron	41·0	4·5
Phosphoric acid	26·4	2·9
	98·4	

Artificial protophosphate of iron he found composed of

Water	27·0	
Protoxide of iron	43·6	4·5
Phosphoric acid	27·4	2·6
	98·0	

The perphosphate of iron he found a compound of

Water	24	
Peroxide of iron	37	5·00
Phosphoric acid	38	5·13
	99	

It is obvious from the equivalent numbers which I have added

Its colour was of a fine shining black, like that of pitch, or black sealing wax. Its taste was faint, and somewhat resembling gum, and it adhered to the teeth like that substance. It was very friable, and easily reduced to powder, and its sp. gr. was considerably above that of water.

It was nearly insoluble in alcohol, but readily soluble in water, forming a deep brownish red solution. This solution yielded copious precipitates by the addition of all the acids and all the metallic and earthy salts tried. These precipitates were of a yellowish red or brown colour.

It burned like gum arabic, and left a considerable quantity of carbonate of potash and traces of an earthy salt, probably the carbonate of lime.

From these experiments it is obvious, that this substance was the ulmate of potash, or ulmin in that state of combination in which it has hitherto been most frequently met with.

XI. *Meteoric Iron.*

(To Dr. Thomson.)

SIR,

The following passage is transcribed from Glauber's Operis Mineralis pars prima, Amstelodami, 1652, 12mo. p. 36. It appears to merit insertion in the *Annals of Philosophy.*

"Neque hoc in terra solùm metallica generationis aptissima, sed et in aëre in densis nubibus idem moliuntur: siquidem non infrequenter videmus, non modo exilia animalcula bruchos, erucas, ranas aliaque insecta, istic locorum concepta et exclusa, confertim cum pluviis descendisse, sed et fide dignis testimoniis constat plusquam centenarios lapides, *ferri etiam massas conglomeratarum guttarum specie, egregie malleabilis ex aëre decidisse.* Uti et varii cometæ, aliæque igneæ substantiæ in aëre coacti accenduntur, materia absumpta, emoriuntur, instar arsenicalis fumi delapsæ terram cum suis fœtibus inficiunt, unde multorum lethalium morborum seges felicissima pullulat. Ipsum etiam fulmen et fulgur nil est aliud quam subtile nitrum accensum, quem admodum et cum fragore cadentes lapides in aëre procreantur."

The remarkable resemblance which the concise description of meteoric iron above bears to the Count de Bournon's in the Phil. Trans. for 1802, cannot fail to strike the mineralogical reader.

I do not know whether the subsequent extract from the same work be, or be not, worthy notice. I believe it is generally considered that Fabbroni first ascertained that gold occurs native in a state of purity.

"Vidi aliquando apud mercatorem Belgam hujusmodi auri granum propemodum finum, vel caratorum 24, aliquot pendens the *stockwerke* at Geyer. St. Michael's Mount, near Penzance, is a very remarkable mountain, which exhibits the relations of these *stockwerkes* in a striking manner, as the same veins penetrate into both, and contain the very same minerals; namely, tinstone, apatite, copper pyrites, &c.

" Similar veins, equally remarkable, occur at Conglure near St. Austle, and at Cliggepoint, not far from St. Agnes. At the latter place are some of the celebrated granite dikes, unconformable masses in killas, and without doubt of the same age with the rock in which they occur. Dartmoor is a desert and bare and almost uninhabited place, in which the most interesting thing which I observed is the *Zinnseifen.* The geological relations of Cornwall are very simple, though for want of a sufficient number of accurate observations they have not yet been fully made out. My astonishment at the number, the richness, the extent, and the quality of the tin and copper veins, is not yet over. When I saw the first heap extracted from a vein, I conceived that it must have been obtained from a bed, and only satisfied myself by actual inspection that the ore was really extracted from a vein.

" An object, on which several geologists in England employ themselves in preference, is the study of the formations lying *above the chalk.* To see them, we went to the Isle of Wight. These newer formations are very remarkable. But the separation of the fresh water formations from each other depends merely on the loose stones found in the different beds, and seems to be merely a conclusion which has been borrowed, perhaps, on too slight grounds from the French."

VII. *Remarkable Mineral Spring in Java.*

Mr. Clarke Abel, in his " Narrative of a Journey in the Interior of China," lately published, gives the following account of a mineral spring in Java, which I am induced to transcribe, though the account is unavoidably incomplete, because the quantity of sulphuretted hydrogen gas which passes through the water seems much greater than has ever been observed in any other part of the world.

" These springs are in the midst of a jungle on the right hand side of the road from Sirang to Batavia, and the country for many miles round is a perfect flat. On approaching them, I smelled the sulphureous gas, which they throw out in immense quantities. They are situated on a piece of barren ground, about 50 yards square, composed of a hard rock, which seemed to have been formed by deposition from the springs. In the midst of this space were several small pools of water in great commotion. They so exactly exhibited the appearance of boiling, that I immersed my hand in them with considerable caution, and scarcely credited my feeling when I found them of the temperature of the surrounding atmosphere. The central pool was the largest, having an area of eight or ten feet. The water bubbled up from several parts of its surface. For the sake of ascertaining the cause of these phenomena, I walked in, and discovered its greatest depth to be about three feet. Its bottom was formed of rock, broken into masses of different shapes. On searching immediately under the place where the agitation of the water was most violent, I found a small funnel shaped aperture, the lower part of which was not more than an inch in diameter. Through this sulphuretted hydrogen gas rushed up in such quantity and with so much force, that I could with great difficulty keep my hand close to its orifice."

"On examining the sensible properties of the water on the spot, I found it to be of a dirty white colour, containing a considerable portion of earthy matter in suspension. The smell was that of Harrowgate water. The soil on the margin and at the bottom of these pools is soft, and of a yellowish-grey colour on the surface; but a few inches beneath, it becomes of a rocky hardness and red. At the distance, however, of two or three feet from the pools, the surface itself is equally hard, but of a blue colour, and bearing evident marks of having been at some distant period the seat of agitated water. A loud bubbling noise is distinctly heard on placing the ear close to any part of the barren spot in which they are situated. The natives believe that the water possesses medicinal properties, and that it is especially efficacious in cutaneous diseases."—(P. 40.)

VIII. *Chinese Stone Yu.*

Many of my readers are aware that there is a stone of a greenish white colour, and considerable hardness, to which the Chinese give the name of Yu, and which they prize more than any other stone. It is said to occur in the form of nodules in the bottom of ravines and in the beds of torrents, and in larger masses in the mountains themselves, especially in Yunan, one of the most northern provinces of the empire. It has been long known in this country under the name of Chinese jade or nephrite; but Prof. Jameson, in the last edition of his Mineralogy, vol. i. p. 505, assures us, that it is prehnite. The following are the characters of this mineral as given by Mr. Clarke Abel, in his Narrative, &c. p. 134.

"Its colour is greenish white, passing into greyish green and dark grass green. Internally, it is scarcely glimmering. Its fracture is splintery; splinters white. It is semi-transparent and cloudy. It scratches glass strongly; and is not scratched by, or scratches, rock crystal. Before the blow-pipe it is infusible without addition.

	Sp. gr.
1. Whitish green, marbled with dark green........	3·330
2. Dark green variety..................	3·190
3. Whitish green variety, same as No. 1	3·400
4. Light-coloured greenish white variety	

"The specimens, of which the specific

were all, except the last, furnished me by the kindness of Sir George Staunton. The last is precisely of the same nature as the sceptre sent to his Royal Highness the Prince Regent, and was put into my possession for the purpose of examination by the Hon. Mr. Amherst, to whom it was presented by one of our attendant Mandarins."

The only part of this description which cannot be reconciled to prehnite is the infusibility before the blow-pipe. The specific gravity of the fibrous variety of prehnite is 2·901, its hardness is nearly the same as that of the Yu; and though its fracture is always fibrous, yet I can conceive it to be described by a person not familiar with the external characters, as having a splintery fracture, which is not altogether erroneous. The infusibility before the blow-pipe seems to separate the Yu both from prehnite and from nephrite to which Mr. Abel refers it.

IX. *Temperature of the Bottom of the Sea.*

The following are all the observations on this interesting subject which Mr. Clarke Abel preserved. The rest were lost by the unfortunate shipwreck of the Alceste. The observations in the following table were made in the Yellow Sea.

	North latitude.	East longitude.	Current.	Depth in fathoms.	Air	Temperature of sea at		Difference of temp. between	
						Surface.	Bottom.	Air and surface.	Surface and bottom.
July 23. 8 a. m.	35° 1'	123° 46'	11 miles.	40	76°	74°	65°	2°	9°
24. 12 m.	36 24	122 59	—	15	75	71	67	4	4
25. 8 a. m.	37 30	122 40	—	20	72	67	62	5	5
—. 8 p. m.	— —	— —	—	15	74	69	66	5	3
26. 6 a. m.	37 58	121 34	—	15	74	67	66	7	1
27. 11 p. m.	38 12	120 20	7 miles.	15	75	74	72	1	2

(Ibid. p. 67.)

Mr. Abel states an experiment on this subject by Captain Wauchope of his Majesty's ship the Eurydice, which deserves to be stated here as one of the most striking and instructive upon record. Within a few degrees of the equator during a calm, Capt. Wauchope put his apparatus overboard, and allowed it to descend till it had carried out 1400 fathoms of line. But he estimated the perpendicular depth at 1000 fathoms. The temperature of the surface was at 73°. On drawing up the instrument, he found the inclosed thermometer marking 42°—a difference of temperature between the surface and given depth of 31°.—(Ibid. p. 347.)

X. *Ulmin from a Cork Tree.*

This substance was collected by Dr. Leach from a decayed cork tree on the estate of —— Marriott, Esq. of Wimbledon.

Its colour was of a fine shining black, like that of pitch, or black sealing-wax. Its taste was faint, and somewhat resembling gum, and it adhered to the teeth like that substance. It was very friable, and easily reduced to powder, and its sp. gr. was considerably above that of water.

It was nearly insoluble in alcohol, but readily soluble in water, forming a deep brownish red solution. This solution yielded copious precipitates by the addition of all the acids and all the metallic and earthy salts tried. These precipitates were of a yellowish red or brown colour.

It burned like gum arabic, and left a considerable quantity of carbonate of potash and traces of an earthy salt, probably the carbonate of lime.

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"Vidi aliquando apud mercatorem F[illegible] hujusmodi auri granum propemodum finum, vel carato [illegible] aliquot pendens latones: plerumque mediocrem arenam magnitudine æquant." —(P. 38.)

Sir Humphry Davy observes, in his Elements of Chemical Philosophy, p. 350, "In my first paper on the Decomposition of the Earths, published in 1808, I called the metal from magnesia, magnium, fearing lest, if called magnesium, it should be confounded with the name formerly applied to manganese. The candid criticisms of some philosophical friends have induced me to apply the termination in the usual manner."

Why did not Sir H. likewise substitute *baritium* and *boracion* for *barium* and *boron?* The latter appellations appear to me to be equally improper with magnium.

It would appear from Mr. Halifax's statement, *Annals,* Nov. 1818, that Mr. Bakewell's discovery of prehnite in Gloucestershire had not, until his communication, been made public.— Mr. B. soon after his discovery, announced it in the Philosophical Magazine. I am, Sir,

Yours respectfully, Φ.

XII. *State of the Barometer, Thermometer, and Magnetical Needle at Trönyem (Drontheim), in Norway, from* 1762 *to* 1783, *inclusive.*—(See Clarke's Travels, vol. v. and Part I. Appendix.)

Barometer.			
Highest.		Lowest.	
1762—Dec...... 30	30·68	Jan........ 13	28·55
1763—March... 12	30·64	Dec........ 30	28·64
1764—Feb...... 23	30·70	Jan........ 28	28·63
1765—Feb...... 2	30·55	March..... 25	28·66
1766—Nov. 7	30·57	March..... 27	28·84
1767—Dec..... 25	30·55	Oct........ 26	28·30
1768—Dec. 12, 14	30·59	Dec........ 28	28·57
1769—Oct...... 14	30·70	April...... 12	28·66
1770—April.... 28	30·33	Feb........ 18	28·10
1771—Feb. 9, 17	30·44	Oct........ 14	28·44
1772—March 8, 13	30·26	Dec........ 11	28·77
1773—March... 11	30·59	Feb........ 24	28·42
1774—Dec...... 7	30·86	Feb........ 25	28·57
1775—Jan...... 24	30·64	Feb........ 2	28·53
1776—Jan...... 7	30·46	Feb........ 6	28·25
1777—Feb...... 8	30·50	Oct........ 31	28·61
1778—March... 11	30·55	Feb........ 23	28·62
1779—March... 7	30·52	Dec........ 23	28·77
1780—Dec...... 19	30·61	Oct........ 20	28·32
1781—Jan. 9, 10	30·61	Feb........ 12	28·66
1782—Nov. 8	30·46	Oct........ 19	28·38
1783—March. .. 15	30·35	Feb........ 9	28·70

	Thermometer.				Magnetic Needle.
	Highest.		Lowest.		Var. W.
1762—	July..... 17	70·25°	Feb. .. 13	−10·60°	—
1763—	July..... 10	78·68	Dec.... 27	− 2·00	—
1764—	July..... 17	77·00	Dec.... 23	− 2·00	—
1765—	July..... 30	74·75	Jan. ... 21	− 4·50	—
1766—	June..... 27 July .. 6, 7	80·60	Dec.... 24	0·75	—
1767—	July..... 5	70·25	Feb. .. 14	− 5·10	—
1768—	June..... 15	77·00	March. 2	−10·19	—
1769—	July..... 23	78·68	Dec.... 29	− 2·31	15° 25′
1770—	July..... 25	77·50	Jan.... 7	− 4·00	15 30
1771—	June..... 24	75·30	Jan.. .. 11	−10·75	15 40
1772—	July..... 28	74·75	Feb... 13	− 9·60	16 6
1773—	June..... 18	77·00	Feb.. . 2	1·60	16 40
1774—	June..... 16 July .. 9, 28	70·25	Jan.... 12	− 9·60	16 46
1775—	Aug. 9	79·25	Jan.. . 25	3·90	16 58
1776—	July..... 14	83·75	Jan. 11, 26	0·75	17 30
1777—	July.. 1, 4	78·60	Feb. 16, 18	− 4·00	17 45
1778—	July.. 21, 22	80·40	Feb. .. 20	2·19	17 50
1779—	Aug. 7	79·80	Jan.... 2 Dec.... 22	10·06	18 00
1780—	June..... 20 July..... 19	72·50	Jan. 20, 30	6·10	18 00
1781—	June..... 18 Aug... 8, 9	75·90	Jan.... 4 Dec.... 31	− 2·00	18 24
1782—	July..... 30	75·30	March . 23	6·10	18 30
1783—	July..... 13	80·90	Dec.... 28	−11·31	18 32

The barometer was placed 20½ ells above the level of the sea. The observations were made at noon. The observations upon the thermometer were made during the winter in the forenoon, and during the summer in the afternoon.

In the original tables, the height of the barometer is given in French inches, and that of the thermometer according to Reaumur. In the above, the height of the barometer is reduced to English inches, and that of the thermometer to Fahrenheit's scale.

XIII. *Berzelius's New Work on Mineralogy.—Specific Gravity of Hydrogen.*

This accurate and indefatigable chemist is now engaged in Paris in printing a work on mineralogy, which will be ready for publication by the beginning of May.

The same excellent experimentalist, in conjunction with M. Dulong, has been lately making experiments with the view of ascertaining the sp. gr. of hydrogen and oxygen. They are said to have determined the sp. gr. of hydrogen to be lower than any preceding chemist had found it, or to be very nearly 0·069.

Article X.

Magnetical, and Meteorological Observations. By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 42″ North. Longitude West in time 1′ 20·7″.

Magnetical Observations, 1819.—*Variation West.*

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Feb. 1	8h 35′	24° 34′ 30″	—h —′	—° —′ —″	Owing to the shortness of the days, evening observation discontinued.	
2	8 40	24 38 37	1 15	24 41 22		
3	8 35	24 36 06	1 20	24 43 06		
4	8 40	24 34 40	1 20	24 41 05		
5	— —	— — —	— —	— — —		
6	8 40	24 32 51	1 25	24 40 58		
7	8 40	24 34 30	1 35	24 41 06		
8	8 35	24 33 50	1 20	24 40 03		
9	8 40	24 31 57	— —	— — —		
10	8 40	24 28 53	1 20	24 36 15		
11	8 35	24 34 27	1 20	24 39 55		
12	8 40	24 32 14	1 30	24 38 58		
13	8 45	24 33 09	1 20	24 38 44		
14	8 35	24 34 25	1 40	24 39 15		
15	8 35	24 34 35	— —	— — —		
16	— —	— — —	— —	— — —		
17	8 35	24 35 12	1 20	24 42 12		
18	8 40	24 35 12	— —	— — —		
19	8 35	24 33 57	1 15	24 38 46		
20	8 45	24 47 06	1 15	24 47 13		
21	8 40	24 34 08	1 15	24 38 57		
22	8 35	24 34 45	1 20	24 39 42		
23	— —	— — —	1 20	24 38 54		
24	8 40	24 35 05	1 20	24 37 09		
25	8 35	24 33 54	1 15	24 40 16		
26	8 40	24 35 23	1 20	24 41 01		
27	8 40	24 35 20	1 20	24 39 44		
28	8 35	24 34 58	1 20	24 40 36		
Mean for the Month.	8 38	24 34 17	1 21	24 39 55		

The variation on the 20th, being so much in excess, is not included in the mean variation; and it may not be improper to remark, that at three o'clock the same day, some very dense clouds formed in the west, at the distance of about six miles, which apparently discharged torrents of rain.

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Feb.		Inches.				Feet.		
1	Morn....	29·128	29°	78°	W by S		Very fine	28°
	Noon....	—	—	—	—		—	37½
	Even	—	—	—	—		—	32
2	Morn....	29·070	32	90	NNW		Snow	
	Noon....	29·130	34	70	NNW		Very fine	34½
	Even	—	—	—	—		—	24½
3	Morn....	29·253	27	73	S		Cloudy	
	Noon....	29·156	33	60	SSE		Cloudy	38
	Even	—	—	—	—		—	31½
4	Morn....	29·085	36	66	W by N		Cloudy	
	Noon....	29·190	41	54	WNW		Showery	41
	Even	—	—	—	—		—	33
5	Morn....	29·241	36	84	ESE		Rain, fog	
	Noon....	29·178	—	95	ESE		Rain, fog	44
	Even	—	—	—	—		—	36
6	Morn....	29·155	42	77	W		Very fine	
	Noon....	29·173	48	46	W		Fine	49
	Even	—	—	—	—		—	35
7	Morn....	28·900	36	84	SW		Very fine	
	Noon....	28·931	43	50	W by N		Cloudy	43
	Even	—	—	—	—		—	34
8	Morn....	29·358	36	60	W by N		Cloudy	
	Noon....	29·438	43	48	WNW		Fine	45
	Even	—	—	—	—		—	36½
9	Morn....	29·454	43	95	SSW		Rain	
	Noon....	29·410	45	96	SSW		Rain	50
	Even	—	—	—	—		—	42½
10	Morn....	29·300	44	66	W		Fine	
	Noon....	29·448	46	42	W		Very fine	47
	Even	—	—	—	—		—	36½
11	Morn....	29·663	43	66	W		Fine	
	Noon....	29·560	48	60	W		Cloudy	49
	Even	—	—	—	—		—	42½
12	Morn....	29·248	43	63	W		Cloudy	
	Noon....	29·142	42	56	W by N		Showery	44½
	Even	—	—	—	—		—	34
13	Morn....	29·270	36	57	WNW		Clear	
	Noon....	29·510	45	47	WNW		Cloudy	44½
	Even	—	—	—	—		—	29⅔
14	Morn....	29·516	31	63	NW		Very fine	
	Noon....	29·562	46	44	NNW		Very fine	41
	Even	—	—	—	—		—	31½
15	Morn....	29·546	33	59	SW		Fine	
	Noon....	—	—	—	—		—	33
	Even	—	—	—	—		—	43½
16	Morn....	29·200	—	70	SSW		Rain	
	Noon....	29·100	—	92	SSW		Rain	46
	Even	—	—	—	—		—	41
17	Morn....	28·970	46	66	WSW		Rain	
	Noon....	29·062	50	45	W by S		Very fine	50½
	Even	—	—	—	—		—	[illegible]
18	Morn....	29·083	42	81	NW by N		Cloudy	
	Noon....	—	—	—	—		—	[illegible]
	Even	—	—	—	—		—	

Meteorological Observations continued.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Feb.		Inches.				Feet.		
19	Morn....	28·765	46°	79°	SSW		Rain	41°
	Noon....	28·856	49	50	SW		Fine	50
	Even....	—	—	—	—		—	33
20	Morn....	29·375	35	65	WSW		Very fine	
	Noon....	29·382	45	49	SSW		Fine	46
	Even....	—	—	—	—		—	39½
21	Morn....	28·706	44	69	W by S		Very fine	
	Noon....	28·580	42	57	WSW		Rain	45½
	Even....	—	—	—	—		—	37
22	Morn....	29·315	38	68	N by W		Cloudy	
	Noon....	29·420	43	56	NNW		Showery	43
	Even....	—	—	—	—		—	34⅔
23	Morn....	29·125	—	84	S		Stormy	
	Noon....	28·955	35	82	NW by N		Sleet	40
	Even ...	—	—	—	—		—	30
24	Morn....	29·056	32	62	W by N		Sn. showers	
	Noon....	29·000	37	50	W by N		Cloudy	37½
	Even....	—	—	—	—		—	22
25	Morn....	29·232	33	81	NNW		Snow	
	Noon....	29·293	38	62	NNW		Fine	40
	Even....	—	—	—	—		—	28
26	Morn....	29·194	38	67	WSW		Cloudy	
	Noon....	29·090	35	60	SW		Snow	36
	Even....	—	—	—	—		—	32
27	Morn....	28·901	35	84	S by E		Cloudy	
	Noon....	28·900	42	53	S by W		Cloudy	42
	Even....	—	—	—	—		—	36
28	Morn....	28·820	37	82	E by S		Rain	
	Noon....	28·824	—	74	ESE		Rain	33
	Even....	—	—	—	—		—	

Rain, by the pluviameter, between noon the 1st of Feb. and noon the 1st of March, 2·828 inches. Evaporation, during the same period, 1·430 inch.

⁂ The Editor has been requested by Mr. Adams to insert the following note:

Stonehouse, March 13, 1819.

I have this day been informed by a friend, that my method "for clearing the Lunar Distance," published in your journal for this month, is essentially the same as one given by Captain Robert Heath in the "Supplement to the Royal Astronomer," published in 1768; and I, therefore, hasten to acquaint you of the circumstance, and to beg the favour of your inserting this in your *Annals* for next April. I have never seen the work entitled the "Royal Astronomer," nor heard, either directly or indirectly, of Capt. Heath's method. The publication of this note will, I hope, remove any idea of my having borrowed the principle.

ERRATUM in the same Paper.

In No. LXXV, p. 191, line 23, for 54° 43′ 20″ *read* 58° 43′ 20″.

ANNALS

OF

PHILOSOPHY.

MAY, 1819.

Article I.

Researches on the Measure of Temperatures, and on the Laws of the Communication of Heat. By MM. Dulong and Petit.

(*Concluded from p.* 251.)

Of Cooling in the Air and in Gases.

The laws of cooling in vacuo being known, nothing is more simple than to separate from the total cooling of a body surrounded with air, or with any other gas, the portion of the effect due to the contact of this fluid. For this, it is obviously sufficient to subtract from the real velocities of cooling those velocities which would take place, if the body, *cæteris paribus,* were placed in vacuo. This subtraction may be easily accomplished now that we have a formula which represents this velocity with great precision, and for all possible cases. We can then determine the energy of cooling due to the sole contact of fluids, and such as it would be observed directly if the body could be deprived of the faculty of radiating. This part of our labour required a very considerable number of experiments, because the laws which we wished to discover were to be studied with respect to the different gases, and for each of them at different temperatures, and under different pressures. Each experiment was made and calculated as we have explained above. We shall, therefore, satisfy ourselves with stating the mean results of these different observations.

The first question with which we beh [illegible] py ourselves, was to ascertain whether the modific [illegible] surface of bodies, which produce so powerful an influence on the radiation, occasioned any change in the losses of heat occasioned by the contact of fluids. For this it was sufficient to observe the cooling of our thermometer in a gas of a determinate elasticity and temperature, first with its bulb in the natural state, and then covered with a leaf of silver.

Of all the experiments which had this comparison for its object, we shall only give the two following.

In the first, we observed the cooling of the largest of our thermometers in the balloon containing air under a pressure of 0·72 metre, and at the temperature of 20°.

First Case.—The thermometer being in its natural state.

Excess of temperature of the thermometer.	Total velocities of the cooling of the thermometer.	Velocities of cooling which would have taken place in vacuo.	Velocity of cooling due to the air alone.
200°	14·04°	8·56°	5·48°
180	11·76	7·01	4·75
160	9·85	5·68	4·17
140	8·05	4·54	3·51
120	6·46	3·56	2·90
100	4·99	2·72	2·27

Second Case.—Bulb of thermometer silvered.

Excess of temperature of the thermometer.	Total velocities of the cooling of the therm.	Velocity of cooling in vacuo.	Velocity of cooling due to the air.
200°	6·93°	1·50°	5·43°
180	6·02	1·23	4·79
160	5·19	1·00	4·19
140	4·32	0·80	3·52
120	3·50	0·62	2·88
100	2·80	0·48	2·32

We see, by comparing the last columns of the two preceding tables, that the corresponding numbers deviate so little, that the deviation may with propriety be ascribed to errors in the experiments. Air then, other things being the same, takes away the same quantity of heat from vitreous and metallic surfaces.

The two following tables contain all the elements of a similar comparison made with hydrogen gas. The small thermometer in these experiments was substituted for the large one. The experiments were made at the temperature of 20°, the gas being subjected to a pressure of 0·74 metre.

First Case.—Thermometer in its natural state.

Excess of the temperature of the thermometer.	Total velocities of the cooling of the thermometer.	Velocities of cooling in vacuo.	Velocity of cooling due to the gas.
80°	22·96°	5·03°	17·93°
60	16·14	3·54	12·60
40	9·87	2·18	7·69
20	4·28	0·95	3·33

Excess of the temperature of the thermometer.	Total velocities of the cooling of the thermometer.	Velocities of cooling in vacuo.	Velocities of cooling due to the gas.
80°	19·59°	1·77°	17·82°
60	13·97	1·29	12·68
40	8·62	0·87	7·75
20	3·74	0·37	3·37

This comparison gives for hydrogen a result similar to that for air. The equality being thus verified for surfaces differing so much from each other as glass and silver, and for gases of such different qualities as air and hydrogen, it is natural to generalize the result, and to deduce from it the following law.

The loss of heat owing to the contact of a gas, other things being equal, is independent of the state of the surface of the body which cools.

This remarkable law of the communication of heat has been already admitted by Mr. Leslie. But this skilful philosopher has only given it as a probable consequence of two indirect experiments, which consist in proving that the state of the surface has only a very feeble effect on the time of cooling in those circumstances in which radiation can contribute but a very small portion of the loss of heat. This is the case, for example, when a hot body is exposed to a violent wind, or when it is plunged into a liquid. But these experiments, however ingenious, can never completely supply the place of direct observations. And in the present case would it not have been possible, for example, to suppose that a property observed in air while violently in motion, could only be applied in a limited sense to air in a state of rest? This doubt would appear still better founded, or would be changed into certainty, if we admitted with Mr. Leslie that air in a state of rest deprives bodies of heat by two different ways; namely, by a conducting property such as exists in solids, and by the renewal of the fluid from ascending currents. Our process, by enabling us in the first place to show the existence of the same law in different gases, dissipates all the doubts which the experiments of Mr. Leslie still allowed to remain. This is one of the cases in which the advantages of the uniform method which we have adopted can be best seen.

The principle which we have just established being well verified, we may confine ourselves in the remainder of our experiments to observe the cooling of the thermometer with the bulb naked in air and the different gases. Henceforth we shall give only in our tables the effect produced solely by the contact of the gas. They have been always calculated, as we have said before, by subtracting from the total velocities of cooling, those which would take place in the same circumstances if the thermometer were cooling in vacuo.

We shall now enter into the examination of the different circumstances which may modify the action of the elastic fluids in the production of the phenomenon which occupies our attention. We shall study the influence of each of these causes, first in air, then in hydrogen, carbonic acid, and olefiant gases. We made choice of the first two, in consequence of the great difference of their physical properties. Air and olefiant gas, on the contrary, offer the curious comparison of two gases of almost the same specific gravity, but of very different composition. The example of the influence of the temperature of the surrounding medium has upon the rate of cooling in vacuo, naturally led us to examine in the first place, if the temperature of the gases does not produce an analogous effect upon the quantity of heat which they carry off. It is needless to say that such experiments had not yet been attempted, the philosophers who have turned their attention to this subject having always supposed that the velocities of cooling depend only on the excess of temperature.

Without stopping to detail our first attempts, we shall state immediately those tables in which the law shows itself manifestly. In the experiments in question, the temperature of the gases was varied by heating sufficiently the water of the balloon; but the gas was at the same time allowed to dilate itself, so that it always preserved the same elasticity. The following table contains the results of such a set of experiments made upon air.

Excess of the thermometer above the surrounding air.	Velocities of cooling due to the contact of air under the pressure 0·72m. temp. 20°.	Ditto pressure 0·72 m. temp. 40°.	Ditto temp. 60°.	Ditto temp. 80°.
200°	5·48°	5·46°	—	—
180	4·75	4·70	4·79	—
160	4·17	4·16	4·20	4·13
140	3·51	3·55	3·55	3·49
120	2·90	2·93	2·94	2·88
100	2·27	2·28	2·24	2·25
80	1·77	1·73	1·71	1·78
60	1·23	1·19	1·18	1·20

The mere inspection of this table is sufficient to show us that the velocities of cooling have remained the same in each of the four series for the same excess of temperature. This simple law was of too great importance not to endeavour to verify it with other gases. The following table exhibits a similar comparison for hydrogen gas, heated successively to 20°, 40°, 60°, and 80°. The elasticity in each experiment was 0·72 metre.

Excess of temp. of therm. above the surrounding gas.	Velocities of cooling due to the contact of the gas at 20°.	Ditto at 40°.	Ditto at 60°.	Ditto at 80°.
160°	14·26°	14·08°	14·18°	—
140	12·11	12·16	12·12	12·08
120	10·10	10·13	10·20	10·19
100	7·98	7·83	8·03	8·05
80	6·06	5·97	6·01	6·00
60	4·21	4·17	4·18	4·20

This table leads to the same consequences as the preceding. To show that it extends to all the gases, whatever be their nature or density, we shall add here a similar set of experiments on carbonic acid, under a pressure of 0·72 metre, and on dilated air under a pressure of 0·36 metre.

Excess of temperature.	Velocities of cooling due to the carbonic acid; temp. 20°.	Ditto temp. 40°.	Ditto temp. 60°.	Ditto temp. 80°.
200°	5·25°	5·17°	—	—
180	4·57	4·63	4·52	—
160	4·04	4·06	3·97	4·10
140	3·39	3·39	3·34	3·43
120	2·82	2·80	2·79	2·83
100	2·22	2·18	2·21	2·20

Excess of temperature.	Velocities of cooling due to dilated air; temp. 20°.	Ditto temp. 40°.	Ditto temp. 60°.	Ditto temp. 80°.
200°	4·01°	4·10°	—	—
180	3·52	3·50	3·55	—
160	3·03	2·99	3·04	3·09
140	2·62	2·57	2·62	2·66
120	2·12	2·16	2·14	2·15
100	1·69	1·71	1·67	1·73

From all these comparisons, we may deduce the following law:

The velocity of cooling of a body, owing to the sole contact of a gas, depends, for the same excess of temperature, on the density and temperature of the fluid; but this dependance is such that the velocity of cooling remains the same, if the density and the temperature of the gas change in such a way that the elasticity remains constant.

Hence in experiments on cooling by the gases, we need only attend to their elasticity. It is, therefore, the influence of this last element that we must endeavour to appreciate.

With this view we have determined for each gas, at different elasticities, the velocities of cooling for the same excesses of temperature. Of each of these series of experiments, we shall merely give what is necessary to demonstrate the law which we have obtained.

Let us begin with air.

The following table exhibits the corresponding velocities of cooling, owing to the sole contact of air under the following pressures; 0·72 m. 0·36 m. 0·18 m. 0·09 m. 0·045 m.; that is to say, under pressures decreasing as the numbers 1, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$.

Excess of temperature above the air.	Velocity of cooling. Pressure 0·72 m.	Ditto. Pressure 0·36 m.	Ditto. Pressure 0·18 m.	Ditto. Pressure 0·09 m.	Ditto, Pressure 0·045 m.
200°	5·48°	4·01°	2·95°	2·20°	1·59°
180	4·75	3·52	2·61	1·90	1·37
160	4·17	3·03	2·21	1·62	1·20
140	3·51	2·62	1·91	1·40	1·02
120	2·90	2·12	1·57	1·15	0·84
100	2·27	1·69	1·23	0 90	0·65
80	1·77	1·29	0·96	0·70	0·52
60	1·23	0·90	0·65	0·48	0·35
40	0·75				
20	0·32				

If we take the ratios of the corresponding number in the second and third columns, we find their values commencing with the uppermost as follows:

1·37 .. 1·35 .. 1·37 .. 1·34 .. 1·37 .. 1·34 .. 1·37 .. 1·36

We have likewise for the ratios of the numbers in the third and fourth columns;

1·36 .. 1·35 .. 1·37 .. 1·37 .. 1·35 .. 1·37 .. 1·34 .. 1·37

The ratios between the fourth and fifth columns are:

1·34 .. 1·37 .. 1·36 .. 1·36 .. 1·37 .. 1·36 .. 1·37 .. 1·35

And lastly, the ratios between the fifth and sixth columns are:

1·38 .. 1·38 .. 1·35 .. 1·37 .. 1·36 .. 1·37 .. 1·35 .. 1·37

The small irregularities which these corresponding ratios present in the numbers which represent them, being less than the uncertainty of the observations, we have a right to draw from them the following conclusions:

1. *The law, according to which the velocity of cooling by the contact of air varies with the excess of temperature, continues the same, whatever the elasticity of the air is.*

2. *The elasticity of the air varying in a geometrical progression, its cooling power changes likewise in a geometrical progression in such a manner that when the ratio of the first geometrical progression is 2, that of the second is* 1·366, *the mean of all the numbers given above.*

It will be easily seen that the law just announced was not recognized till we had made many experiments. But when once verified for air, it was natural to try it on the other gases. We shall now give a tabular view of the observations relative to each of these.

Let us begin with hydrogen.

Excess of temperature above the gas.	Velocity of cooling due to the contact of hydrogen. Pres. 0·72 m.	Ditto. Pressure 0·36 m.	Ditto. Pressure 0·18 m.	Ditto. Pressure 0·09 m.	Ditto. Pressure 0·045 m.
180°	16·59°	12·86°	9·82°	7·49°	5·81°
160	14·26	10·97	8·37	6·49	4·95
140	12·11	9·24	7·11	5·47	4·24
120	10·10	7·83	5·99	4·64	3·51
100	7·98	6·23	4·79	3·63	2·80
80	6·06	4·62	3·58	2·77	2·09
60	4·21	3·21	2·48	1·88	1·46

The ratios between the numbers in the second and third columns are:

1·29 .. 1·30 .. 1·31 .. 1·29 .. 1·28 .. 1·31 .. 1·31

The ratios between the numbers of the third and fourth columns are:

1·31 .. 1·31 .. 1·30 .. 1·31 .. 1·32 .. 1·29 .. 1·29

The ratios between the numbers of the fourth and fifth columns are:

1·31 .. 1·29 .. 1·30 .. 1·29 .. 1·30 .. 1·29 .. 1·32

The ratios between the numbers of the fifth and sixth columns are:

1·29 .. 1·31 .. 1·29 .. 1·32 .. 1·30 .. 1·32 .. 1·29

The very near approach to equality in these [illegible] furnishes us with a result analogous to that which is relat[illegible] Hence

1. *The law, according to which the velocity of cooling due to the sole contact of hydrogen varies with the excesses of temperature, is the same, whatever be the elasticity of the gas.*

2. *The cooling power of hydrogen decreases in a geometrical progression, whose ratio is* 1·301, *when its elasticity diminishes in a geometrical progression, whose ratio is* 2.

We obtained the same consequences for carbonic acid and olefiant gas. This may be easily verified in the two following tables, arranged for each of these gases as the table given above for hydrogen.

Excess of temperature above the gas.	Velocities of cooling due to carbonic acid. Pres. 0·72 m.	Ditto. Pressure 0·36 m.	Ditto. Pressure 0·18 m.	Ditto. Pressure 0·09 m.	Ditto. Pressure 0·045 m.
200°	5·25°	3·64°	2·56°	1·79°	1·25°
180	4·57	3·22	2·25	1·56	1·09
160	4·04	2·80	1·97	1·37	0·95
140	3·39	2·38	1·65	1·17	0·80
120	2·82	1·97	1·36	0·95	0·67
100	2·22	1·55	1·08	0·76	0·52
80	1·69	1·17	0·82	0·57	0·40
60	1·18	0·82	0·57	0·40	0·28

Excess of temperature of therm. above the gas.	Velocities of cooling due to the contact of olefiant gas. Pres. 0·72 m.	Ditto. Pressure 0·36 m.	Ditto. Pressure 0·18 m.	Ditto. Pressure 0·09 m.	Ditto. Pressure 0·045 m.
200°	7·41°	5·18°	3·64°	2·58°	1·84°
180	6·45	4·57	3·17	2·22	1·59
160	5·41	3·86	2·72	1·89	1·34
140	4·70	3·31	2·35	1·63	1·18
120	3·84	2·76	1·92	1·35	0·96
100	3·12	2·21	1·55	1·08	0·78
80	2·34	1·62	1·15	0·79	0·62

Mean of all these Ratios.

For carbonic acid .... = 1·431
For olefiant gas .... = 1·415

From all that precedes, we may draw the following consequences:

1. *The losses of heat due to the contact of a gas increase with the excess of temperature, according to a law which remains the same whatever be the elasticity of the gas.*

2. *The cooling powers of the same gas vary in geometrical progression, while the elasticities vary in geometrical progression; and if we suppose the ratio of this second progression to be* 2, *the ratio of the first progression will be* 1·366 *for air;* 1·301 *for hydrogen;* 1·341 *for carbonic acid; and* 1·415 *for olefiant gas.*

This result may be announced in a manner still more simple, to which we are led by the following calculation.

If we call P the cooling power of air under the pressure p, this power will become P (1·366) under a pressure $2\,p$; $P\,(1·366)^2$ under a pressure $4\,p$; and under a pressure $p\,.\,2^n$, it will be $P\,(1·366)^n$. Making $p\,.\,2^n = p'$ and $P\,(1·366)^n = P'$, we shall obviously obtain by eliminating n.

$$\frac{\text{Log. } P' - \text{log. } P}{\text{Log. } (1·366)} = \frac{\text{Log. } p' - \text{log. } p}{\text{Log. } 2}$$

Hence

$$\frac{P'}{P} = \left(\frac{p'}{p}\right)^{0·45}$$

We shall find in the same way for hydrogen

$$\frac{P'}{P} = \left(\frac{p'}{p}\right)^{0·38}$$

For carbonic acid, the exponent will be 0·517, and for olefiant gas 0·501.

From this we conclude, that the cooling power of a gas is, every thing else being equal, proportional to a certain power of its elasticity; but that the exponent of this power varies from one gas to another. It is 0·38 for hydrogen, 0·45 for air, 0·517 for carbonic acid, and 0·501 for olefiant gas. These last three numbers differing little from 0·5, we may say that in the gases to which they belong, the cooling power is nearly as the square root of the elasticity.

If we compare the law which we have thus announced with the approximations of Leslie and Dalton, we shall be able to judge of the errors into which they have been led by the inaccurate suppositions which serve as the basis of all their calculations, and by the little precision attainable by the methods which they have followed. The first by photometrical experiments, calculated by the law of Newton, finds the cooling power of air proportional to the fifth root of its density; and Mr. Dalton finds it proportional to the cube root, supposing, as he always does, the law of cooling the same for all bodies and in all the gases.

Now that we know the influence that the temperature and the density of the gas in which it takes place has upon cooling, it remains to discover how for a given state of a fluid the velocities of cooling depend upon the excesses of the temperature.

We have already observed, that the law which expresses this dependance remains the same for the same gas when its elasticity changes. Let us see now what happens when we pass from one gas to another; and for this purpose let us resume, from the preceding tables, the velocities of cooling due to the sole contact of air, of hydrogen, carbonic acid, and olefiant gases, these four fluids being under a pressure of 0·72 metre.

Excesses of temp. of therm. above the surrounding fluid.	Velocities of cooling due to the contact of air.	Ditto of hydrogen.	Ditto of carbonic acid.	Ditto of olefiant gas.
200°	5·48°	—	5·25°	7·41°
180	4·75	16·59	4·57	6·45
160	4·17	14·26	4·04	5·41
140	3·51	12·11	3·39	4·70
120	2·90	10·10	2·82	3·84
100	2·27	7·98	2·22	3·12
80	1·77	6·06	1·69	2·34

On dividing the numbers in the third column by those in the second, we find for the ratios between the losses from hydrogen and those from air

3·49 .... 3·42 .... 3·45 .... 3·48 .... 3·51 .... 3·43

Now as it would be sufficient to render these ratios equal to alter the velocities which have served to determine them by quantities within the limits of the uncertainty to which all such experiments are exposed, we may conclude that the law is the same for hydrogen and for air.

We shall come to a similar conclusion for the two other gases, if we take the ratios of the velocities of cooling which they produce to the corresponding velocities produced by air. The numbers for carbonic acid are,

0·958 .. 0·962 .. 0·968 .. 0·965 .. 0·972 .. 0·977 .. 0·955

Those for olefiant gas are,

1·35 .. 1·36 .. 1·30 .. 1·33 .. 1·32 .. 1·37 .. 1·32

The law of cooling produced by the sole contact of a gas is then independent of the nature and density of this gas; and the comparison of the series given above, with an analogous series of cooling in vacuo, shows clearly that the law of which we are in search differs from that of radiation. After a great many trials, of which it would be superfluous to give an account, we have found that the velocities of cooling due to the sole contact of a gas vary with the excesses of temperature of the body, according to a law analogous to that which connects the cooling power of a fluid with its elasticity; that is to say, that the quantities of heat which a gas carries off from a body increase in a geometrical progression, while the excesses of temperature likewise increase in a geometrical progression. The ratio of this last progression being 2, that of the first is 2·35. We deduce likewise, by a calculation similar to those formerly employed, that the losses of heat due to the contact of a gas are proportional to the excesses of temperature of the body elevated to the power 1·233.

To enable the reader to judge of the accuracy of this law, we shall give in the following table, the velocities of cooling produced by the contact of air under a pressure of 0·72 m.; the second column containing the values of these velocities observed; and the third, their values deduced from the law which we have announced.

Excesses of temperature.	Velocities observed.	Ditto calculated.
200°	5·48°	5·45°
180	4·75	4·78
160	4·17	4·14
140	3·51	3·51
120	2·90	2·91
100	2·27	2·31
80	1·77	1·76
60	1·23	1·24
40	0·77	0·75
20	0·33	0·32

It is needless to transcribe the similar comparisons which we have made on the other gases, and each of the pressures under which we have operated; for we have recognized above, that the series relative to each of them follow exactly the same law as for air, and that this law is observed under all pressures. But the comparisons of which we speak have afforded us as satisfactory results as the preceding; and indeed this may be easily verified upon each of the series of observations which we have given above.

To obtain a general expression of the velocity of cooling due to the contact of a fluid, it is necessary to collect all the particular laws which we have made known. But the first law informs us that the state of the surface of the body has no influence on the quantity of heat which the fluid carries off from it, and the second law proves that the density and the temperature of this fluid do not affect the cooling but in as far as they contribute to vary the pressure; so that the cooling power of the fluid depends ultimately upon its elasticity. This elasticity and the excess of temperature of the body are then the two only elements which can make the velocity of cooling vary. Denoting the first of these elements by p, and the second by t, we shall have for V the velocity by the contact of a fluid.

$$V = m \cdot p^c \cdot t^b$$

b being for all gases and all bodies equal to 1·233; c being likewise the same for all bodies, but varying from one gas to another; and m having a value which changes with the nature of the gas and with the size of the body. The values of c are, as we have found, 0·45 for air; 0·38 for hydrogen; 0·517 for carbonic acid; and 0·501 for olefiant gas. The values of m depend, as we have said, on the dimensions of the body and the nature of the gas; for our thermometer m is equal to 0·00919 in air; to 0·0318 in hydrogen; to 0·00887 in carbonic acid; and to 0·01227 in olefiant gas. (These values of m suppose expressed in metres, and t in centigrade degrees.) We may, by the preceding value of V, calculate the ratios of the cooling powers of the different gases for each pressure. Thus taking the cooling power of air at unity, and supposing the pressure = 0·76 m, we have for the cooling power of hydrogen 3·45, and for that of carbonic acid 0·965. These numbers will change with the elasticity belonging to the three gases. This Messrs. Leslie and Dalton did not perceive; but it is easily deduced from our formula. However, their determinations differ but little from those which we have calculated for the pressure of 0·76 m. We should deduce likewise ratios very little different from these from the experiments made more recently by Sir Humphry Davy.

The simplicity of the general law which we have just made known made us desire eagerly to be able to verify it at temperatures more elevated than those which we had attempted in our experiments. We succeeded by a very simple process, the idea of which was first suggested by Mr. Leslie.

When our thermometer with the naked ball cooled in the open air, the total velocity of this cooling is the sum of the velocities due separately to the contact of air and to radiation. Denoting these by v and v', the total velocity is $v + v'$. If the thermometer be covered with silver, the velocity v due to the air remains the same for the same temperature, and v' is reduced to $\frac{v'}{5{\cdot}707}$, since the constant ratio of the radiating powers of glass and silver is 5·707. The total cooling of the silvered thermometer is then $v + \frac{v'}{5{\cdot}707}$. Hence it is easy to conclude that in order to know at all temperatures the losses of heat produced by the contact of air, it is sufficient to determine the total velocities of cooling of our thermometer, first when the bulb is naked, and then when it is covered with silver. These velocities being represented by a and b, we shall have

$$a = v + v' \qquad b = v + \frac{v'}{5{\cdot}707}$$

$$\text{Hence } v = \frac{5{\cdot}707 \times b - a}{4{\cdot}707}$$

Let us apply this formula to the results contained in the following table:

Excesses of temp. of thermometer.	Total velocities of cooling of the naked bulb.	Total velocities of cooling of the silvered bulb.	Values of V.
260°	24·42°	10·96°	8·10°
240	21·12	9·82	7·41
220	17·92	8·59	6·61
200	15·30	7·57	5·92
180	13·04	6·57	5·19
160	10·70	5·59	4·50
140	8·75	4·61	3·73
120	6·82	3·80	3·11
100	5·57	3·06	2·53
80	4·15	2·32	1·93

The second and third columns contain the total velocities of cooling of a thermometer with a naked and silver bulb for the excesses of temperature contained in the first column. The last column contains the corresponding values of v; that is to say, the losses of heat, which the contact of air alone produces in both thermometers. But the law which these losses of heat follow is expressed by the following equation:

$$v = n\,t^{1{\cdot}233}$$

in which n must be determined in each particular case; for the one which we are considering $n = 0{\cdot}00857$. By giving successively to t all the values for every 20°, from 80° to 260°, we shall have the corresponding values of v, which will differ but little from those deduced experimentally. To make this comparison more easy, we have united in the following table the observed and calculated values of v.

Excesses of temp.	Observed values of V.	Calculated values of V.
260°	8·10°	8·14°
240	7·41	7·38
220	6·61	6·63
200	5·92	5·87
180	5·19	5·17
160	4·50	4·47
140	3·73	3·79
120	3·11	3·14
100	2·53	2·50
80	1·93	1·90

Thus the losses of heat by air are confirmed when we extend our observations to greater excesses of temperature. The results already stated will likewise furnish us with the means of verifying the law of cooling in vacuo. I[illegible]cient for that to subtract from the total velocities o[illegible] those which are due to the sole contact of air; tha[illegible]essive values of v. The remainders will evidently be the velocities of cooling owing to radiation, or, which comes to the same thing, those which would have taken place in vacuo.

We give here the numbers thus determined for the thermometer with its bulb naked; we join to them the velocities deduced from the law of cooling in vacuo. The velocity in this case is expressed by

$$m(a^t - 1);$$

t representing the excess of temperature of the body, m a constant coefficient which must be determined in each case, and which is here equal to 2·61; a denoting the exponent 1·0077 common to all bodies.

Excesses of temp.	Velocities of cooling in vacuo deduced from observations in air.	Velocities of cooling in vacuo by calculation.
260°	16·32°	16·40°
240	13·71	13·71
220	11·31	11·40
200	9·38	9·42
180	7·85	7·71
160	6·20	6·25
140	5·02	4·99
120	3·93	3·92
100	3·04	2·99
80	2·22	2·20

We see, from the example which we have just given, that it is possible, by immediate observations of cooling in air, to estimate separately the losses of heat due to contact and to radiation; and that it is necessary for this to observe the cooling of the same body under two different conditions of surface. But this mode of calculation depends on the one side on the supposition that the quantity of heat carried off by the air is independent of the nature of the surface of the body; and on the other on this principle, that bodies of a different nature preserve at all temperatures the same ratio between their radiating powers. These two propositions are rigorously true, but can only be constated by direct experiments, such as those which we have stated above; and though Mr. Leslie has adopted them in the use which he has made of the principle which we have just explained, his results have not all the accuracy that could be desired, because he has always calculated the velocities of cooling according to the Newtonian law.

The laws relative to each of these two effects which concur to the cooling of a body plunged into a fluid being separately established, it is merely necessary to unite them in order to deduce the law of total cooling.

The velocity v of this cooling for an excess t of temperature will be then expressed by the formula

$$m(a^t - 1) + n\,t^b.$$

The quantities a and b will be for all bodies and in all fluids qual, the first to 1·0077, and the second to 1·233. The coefficient m will depend on the size and the nature of the surface, as vell as upon the absolute nature of the surrounding body. The oefficient n, independent of this absolute temperature, as well s of the nature of the surface of the body, will vary with the lasticity and with the nature of the gas in which the body is lunged; and these variations will follow laws which we have lready established.

This formula shows us in the first place, as we have announced t the commencement of this memoir, that the law of cooling in lastic fluids changes with the nature of the surface of the body. n fact, when this change takes place, the quantities a, b, and n, reserve their values; but the coefficient m varies proportionally o the radiating power of the surface. If we represent its new alue by m', the velocity of cooling will become

$$m' (a^t - 1) + n t^b ;$$

quantity which does not remain proportional to

$$m (a^t - 1) + n t^b,$$

hen t changes.

Let us now examine how the ratio of these two velocities aries, and let us suppose, in order to fix our ideas, that m is reater than m'; that is to say, that it belongs to the body which adiates most.

We may in the first place satisfy ourselves by means of the ules of the differential calculus, that the fraction

$$\frac{m (a^t - 1) + n t^b}{m' (a^t - 1) + n t_b}$$

ecomes equal to $\frac{m}{m'}$, whether we make $t = 0$, or $t = \infty$.

If we suppose t very small, the quantity $a^t - 1$ is reduced to . log. a, and the preceding ratio becomes, dividing by t log. a,

$$\frac{m + \frac{n}{\log. a} . t^{b-1}}{m' + \frac{n}{\log. a} . t^{b-1}}$$

Under this form it is evident that the ratio must diminish in roportion as t increases, b being greater than 1; but this ratio, fter having diminished, will again increase, since it must resume o infinity the value which it has when $t = 0$. From this it is asy to conclude the truth of the principle which we have estalished at the beginning of this memoir, and which comes to his, that when we compare the laws of cooling in two bodies vith different surfaces, the law is more rapid at low temperatures or the body which radiates the least; and less rapid, on the ontrary, for the same body, at high temperatures.

This may be easily ve [illegible] the following table, where we ave inserted the velociti [illegible] ling of the naked thermometer, and of the silvered thermometer, and the ratios between these velocities

Excesses of temp. of thermometer.	Velocities of cooling of the naked thermometer.	Velocities of cooling of the silvered thermometer.	Ratios of these velocities.
260°	24·42°	10·96°	2·23°
240	21·12	9·82	2·15
220	17·92	8·59	2·09
200	15·30	7·57	2·02
180	13·04	6·57	1·98
160	10·70	5·59	1·91
140	8·75	4·61	1·89
120	6·82	3·80	1·80
100	5·56	3·06	1·81
80	4·15	2·32	1·78
60	2·86	1·60	1·79
40	1·74	0·96	1·81
20	0·77	0·42	1·85
10	0·37	0·19	1·90

The mere inspection of the numbers inserted in the last column fully confirms the fact announced above. We perceive likewise the ratios of the velocities of the two thermometers remaining nearly the same for the excesses of temperature between 40° and 120°. This circumstance, resulting obviously from the ratios increasing, after having diminished, has probably contributed to persuade Mr. Dalton that the law of cooling in air must be the same for all bodies. If the above series were carried further, we should find that the ratio of the velocities of cooling which is already equal to 2·23 for an excess of temperature of 260°, increases rapidly as that excess augments, and that it approaches more and more to the number 5·707 to which the fraction $\frac{m}{m'}$ is equal in the case of glass compared with silver. We see from this to what a degree the consequences deduced by Mr Leslie, from experiments made at low temperatures, are inaccurate. For having imagined, as we have said in the beginning of this memoir, that the ratio which we have determined above would continue always to diminish, he had supposed that it would terminate by becoming almost equal to unity; so that at high temperatures, the total losses of heat would be almost independent of the state of the surfaces. The laws which this philosopher has proposed, and likewise those of Dalton and Martine, may be all refuted by a single argument; for all these laws make the velocity of cooling depend solely on the excess of the temperature of the body above that of the surrounding medium; while experience proves that other things being equal, this velocity changes in a remarkable degree with the temperature of the fluid which surrounds the body.

It is needless, therefore, to enter into any discussion on this subject; for, admitting that the laws of which we have just spoken represent the results of experience within the limits in which they have been determined, it is certain, from all that precedes, that when we extend them beyond these limits, we arrive at results very different from the truth.

We may, by considerations analogous to those which we have used above, determine in what manner the law of total cooling changes for the same body with the nature and density of the gases.

The total velocity of cooling is expressed by

$$m\,(a^t - 1) + n\,t^b$$

If we consider another gas, or the same gas, at a different density, the velocity of cooling will be for the same body

$$m\,(a^t - 1) + n'\,t^b$$

for the coefficient n is the only part of the expression which changes in this case.

On comparing these two expressions, we find that their ratio becomes equal to unity, whether we make $t = 0$, or $t = \infty$. Hence the total velocities of cooling in different gases approach equality at very high and very low temperatures; while in the intermediate part of the scale these velocities may be very different. This result is sufficient to show the inaccuracy of the processes which Mr. Dalton and Mr. Leslie employed to compare the losses of heat due to different gases; for these processes are founded on the supposition that the total velocities of cooling in the different gases preserve the same ratio at all temperatures. But from a very singular circumstance, upon which it is needless to insist, the particular temperature at which they operated renders the error very small, and they were far from ascribing it to their mode of calculation. Accordingly their determinations, as we have said before, are very near the truth, provided they be restrained to the circumstances in which they have been made.

The necessity of estimating separately the influence of each of the causes which modify the progress of the cooling of a body not having allowed us to bring together the different laws to which we have come, we conceive that a summary recapitulation will be so much the more useful, because we shall have it in our power to re-establish the natural order which the description of experiments and the discussion of the results have often obliged us to interrupt.

Distinguishing, as we have done, the losses of heat due separately to the contact of fluids and to radiation, we soon perceive that each of these two effects is subject to particular laws. These l[illegible]t to express the relations which exist [illegible] the t[illegible]e of the body and the velocity of its cooling for all possible circumstances. We must recollect that by *velocity of cooling* we mean always the number of degrees which the tempetature of the body would sink during an infinitely small and constant interval of time.

First Law.—If we could observe the cooling of a body placed in a vacuum surrounded by a wall, totally destitute of heat or deprived of the faculty of radiating, the velocities of cooling would decrease in a geometrical progression, while the temperatures diminished in an arithmetical progression.

Second Law.—For the same temperature of the walls of the vacuum in which the body is placed, the velocities of cooling for excesses of temperature in arithmetical progression decrease as the terms of a geometrical progression, diminished by a constant number. The ratio of this geometrical progression is the same for all bodies, and is equal to 1·0077.

Third Law.—The velocity of cooling in vacuo for the same excess of temperature, increases in a geometrical progression, while the temperature of the walls of the vacuum increases in an arithmetical progression. The ratio of this progression is likewise 1·0077 for all bodies.

Fourth Law.—The velocity of cooling due to the sole contact of a gas is entirely independent of the nature of the surface of the body.

Fifth Law.—The velocity of cooling due to the sole contact of a fluid varies in a geometrical progression, while the excess of temperature itself varies in a geometrical progression. If the ratio of this second progression be 2, that of the first is 2·35, whatever be the nature of the gas and its elastic force. This law may be likewise announced by saying, that the quantity of heat carried off by a gas is in all cases proportional to the excess of the temperature of the body raised to the power 1·233.

Sixth Law.—The cooling power of a fluid diminishes in a geometrical progression when its tension itself diminishes in a geometrical progression. If the ratio of this second progression is 2, the ratio of the first is 1·366 for air; 1·301 for hydrogen; 1·431 for carbonic acid; and 1·415 for olefiant gas.

This law may likewise be presented in the following manner:

The cooling power of a gas is, all other things being equal, proportional to a certain power of the pressure. The exponent of this power, which depends on the nature of the gas, is 0·45 for air; 0·315 for hydrogen; 0·517 for carbonic acid; and 0·501 for olefiant gas.

Seventh Law.—The cooling power of a gas varies with its temperature in such a manner that if the gas can dilate, and if it preserves always the same elastic force, the cooling power will be as much diminished by the rarefaction of the gas as it is increased by its augmentation of temperature; so that ultimately it depends only on its tension.

We see from these propositions that the total law of cooling, which would be compounded of all the preceding laws, must be very complicated; we shall not, therefore, attempt to translate it into ordinary language. We have given it in the course of the memoir under a mathematical form, which permits us to examine all its consequences. We shall satisfy ourselves with remarking, that it is doubtless to the very complicated nature of this law that we must ascribe the little success of the attempts hitherto made to discover it. It is obvious that we can only arrive at it by studying apart each of the causes which contributes to the total effect.

ARTICLE II.

On the Weight of a Cubic Inch of distilled Water; and the Specific Gravity of Atmospheric Air. By E. W. M. Rice, A.B. M.R.I.A.

(To Dr. Thomson.)

SIR, *Dublin, March* 6, 1819.

IN the following paper I have endeavoured to deduce the true weight of a cubic inch of water, and its specific gravity in relation to atmospheric air, from a comparison of the French experiments, those made by Sir George Shuckburgh, and a theoretical view of the composition of water. In making the necessary corrections in Sir George's actually experimental results, I have presumed to differ from the experimenter and Mr. Fletcher; as it appears to me that the data on which these corrections are founded cannot be supported in the present state of science. Should you esteem the object of this attempt so far accomplished as to be worthy of publicity, your giving it a place in the *Annals* will much oblige,

Yours very truly,

E. W. M. RICE.

On looking over the 49th volume of the Journal de Physique, it appeared to me that Lefevre Gineau's experiments were conducted with so much attention that his determination of the weight of a volume of water could not be far from the truth; and that the difference generally supposed to exist between his and Sir George Shuckburgh's must be chiefly attributed to the imperfection of the data on which the French experiment was stated in English weights, and Sir George's reduced to the mean temperature and pressure.

I think that, for the present, we may reckon the Paris pound at 32° Fahr. equal to 7560 English troy grains at 62°. This I have deduced from Tillet's experiments; as little confidence can be placed in the determi[illegible]s made in 1742, from the roughness of the experiments, and the subsequent change of mint standards. Capt. Kater has lately measured the length of the French standard metre, and found it equal to 39·37079 English longitudinal inches; each standard being at its proper temperature. Hence the cubic decimetre, or litre, will contain 61·0270554 English cubic inches; now this bulk of distilled water was found to weigh, at its maximum of density and in vacuo, 18827·15 gr. of the pile of Charlemagne; and, therefore, one English cubic inch of distilled water, under like circumstances, weighs 308·505 French gr. equivalent to 253·07148 English troy gr.; and taking the expansion of water with Blagden and Gilpin, 1·00094 : 1 :: 253·07148 : 252·8338. From this last number, which expresses the absolute weight of a cubic inch of water at 60° Fahr. let the weight of a cubic inch of air at 60° ther. and 30 bar. (generally accounted about 0·31 gr. but which will hereafter appear to be more accurately 0·30519) be subtracted; the remainder 252·5238 will express the weight of a cubic inch of distilled water at 60° therm. and 30 in. barometrical pressure.

I shall estimate it at 252·525 gr. (a number affording great facility in calculations), and endeavour to show that Sir George Shuckburgh's experiments, and a theoretical view of the composition of water, justify that choice.

The first step here necessary is, from Sir George's experiments made in Savoy (Phil. Trans. for 1777), to find the ratio of air to water when the barometer stands at 30 inches and Fahrenheit's thermometer at 60°. In doing this, I shall not extend the calculations further than two decimal places, as the experiment does not seem to have been made with sufficient precision to warrant greater nicety. Air and water were successively weighed in the same glass globe, or rather flask.

The weight of its contents of air at 53° and 29·27 barometer was ascertained to be 16·22 gr.

Its contents of water at 51° weighed 13562·6 gr.

From the expansibility of glass, it is evident that the capacity of the globe at 53° is greater than at 51°. I have calculated the difference produced by change of temperature on the quantity of air contained in the globe and converted into weight at about 0·002 gr. which is, therefore, to be deducted from 16·22, leaving 16·218, which becomes 16·38 when reduced to a mean temperature and pressure by the following proportions: 1058333 : 1043749 :: 16·218 : 15·99, and 29·2717 (correcting for apparent expansion of mercury) : 30 :: 15·99 : 16·38.

The buoyancy of air at 51° being greater than at 60°, a bulk of brass, whose weight is marked 13554 at the latter temperature, would at the former weigh about 0·03 gr. less; hence the actual weight is less at 51° than at 60° by 0·03 gr.; the apparent weight is, therefore, to be diminished by that quantity.

A bulk of water at 51°, equal to 13562·6 gr. will have its weight at 60° indicated by the following proportion, 1·00063 : 1 :: 13562·6 : 13554·06; from this last number, 0·03 is to be taken, as the correction for buoyancy of air.

Hence the ratio of water at 60 to air at 60° and 30° is $\frac{13554 \cdot 03}{16 \cdot 38}$ = 827·472 : 1, or thereabouts.

If we estimate the specific gravity of oxygen at 1·1111, and that of hydrogen at 0·0694 (both at 60° and 30°), and suppose water to be composed of hydrogen and oxygen united in the proportion of two volumes of the former to one of the latter, the specific gravity 827·472 will indicate the formation of water to be effected by the union of 1324 volumes of hydrogen with 662 volumes of oxygen—the whole condensed into one volume, the specific gravity of which will be 827·4338. This comes so near the former determination that I have little doubt but the assigned is the correct, or at least very nearly correct composition. The specific gravity of air to water would in this case be represented by 0·001208555 : 1; and reckoning the weight of a cubic inch of water at 252·525 gr. a cubic inch of air will weigh 0·30519035 gr. or 0·30519 gr. From this latter number, deducing the weights of the two component gases, the composition of water, weight of a cubic inch, and specific gravity, will stand thus:

	Cubic inches.	Sp. Gr.	Weight Gr.
Hydrogen	1324	91·8856	28·04256
Oxygen	662	735·5482	224·48195
	1986	827·4338	252·52451

I am disposed to think the above a very close approximation to the truth, and that when the thermometer stands at 60° and the barometer at 30, we may estimate 100 cubic inches of dry atmospheric air to weigh 30·519 gr.; one cubic inch of distilled water 252·525 gr.; the specific gravity of water to air as 827·437 : 1, or reckoning water as unity as 0·00120855 : 1; from these data I will endeavour to make the necessary corrections in Sir George Shuckburgh's experiments on the weight of a given volume of water. (Phil. Trans. 1798.) I am aware that the subject has been handled in Nicholson's Journal by a gentleman of great abilities; and am the more confident in the estimates obtained by my calculations, as they approach very near his determinations, and the numbers to which Dr. Thomson's adoption has added so much weight.

I shall only notice the experiments made with the brass sphere: in these the experimenter himself seems to place most confidence; and they were undoubtedly conducted with very little liability to error. As the contents of the sphere seem to me to have been estimated at 64° Fahr. (the temperature at which the length of the bar was valued) to contain 113·519147 cubic inches, and its expansion being for each degree of Fahr.

= three millionth parts of that bulk, its volume at certain temperatures will be as follows:

At 60·0° = 113·519011 cubic inches
64·0 = 113·519147
66·0 = 113·519215
66·4 = 113·519228
67·0 = 113·519249
68·0 = 113·519283

To enable us to make the compensations for temperature and pressure, we must observe that at

Grains.
66° a volume of water equal the sphere weighs 28683·5766
64 .... 28682·4323

Difference for 0·4° therm.. 1·1443

And for 0·1° .... ·2861

The volume of air displaced by the sphere at

Grains.
67° and 29·74 weighs 33·87528
67 and 30·00 ...... 34·17156
68 and 30·13 ...... 34·25217

Difference between the first and third weights = 0·3769, between the first and second 0·3063 gr.

Supposing the weight, or equipoise to the sphere in air, to contain 13·5 cubic inches, it will displace at

Grains.
67° and 29·74 a volume of air weighing 4·0285
68 and 30·13 .... 4·0733
60 and 30·00 .... 4·1200

The weight will thus become of actually less value as a counterpoise at 68° and 30·13, or at 67° and 29·74, than at 60° and 30; but its nominal value remains constant: in the first case, therefore, 0·0915, and in the second 0·0467, is to be subtracted.

Sir George found the weight of the sphere in water at

Grains.
66·0° by the first trial .... 49·8500
66·1 by the second trial = 49·8100
− 00·2866 for 0·1°
49·5234
66·4 by the third trial .. = 49·5500
− 1·1464 for 0·4°
48·4036

Mean. .... 49·2590

And the weight of the same sphere in air at

		Grains.
67° and 29·74 by first trial. .. =	28722·6400	
Cor. for error from buoyancy.. −	00000·0467	
		28722·5933
68° and 30·13 by second trial. =	28721·8800	
For diff. in therm. and barom. +	00000·3769	
Error from buoyancy .... −	00000·0915	
		28722·1654
Same, by third trial.		28722·1654
Mean......		28722·3080
From which take the mean weight of the sphere in water .... −		00049·2590
Weight of a bulk of water = the sphere at 66° and 29·74		28673·0490
	−	00000·3063
66 and 30·00		28672·7427

$\therefore \frac{28672 \cdot 7427}{113 \cdot 519215} = 252 \cdot 580$ is the weight of a cubic inch of distilled water at 66° and 30. But as the weights used were too light, when compared with the Exchequer standard, by 1 in 1523·92, that quantity must be deducted; leaving 252·414 gr. standard; correcting for expansion of water 0·99939 : 1 :: 252·414 : 252·568 at 60° and 30. Now in this state of the atmosphere, it displaces a volume of air = 0·30519 gr.; but at 66° and 30 = 0·30161; ∴ 252·568 being diminished by the difference 0·00358, gives 252·564 for the weight of a cubic inch of distilled water at a mean temperature and pressure. If we suppose the water used in the different trials to be a little impure, so that its specific gravity at 60° = 1·000138, we shall have 252·529 as the true weight in standard grains of a cubic inch of the purest distilled water, in a temperature of 60° and under a pressure of 30 in. This numbers differs but very little from 252·525.

The reason for pitching on the number 1·000138 to represent the specific gravity is, that Sir George Shuckburgh, after one of his experiments, found the distilled water used to weigh at 60° 1·00055, allowing half the increase indicated by the hydrometer to be caused by inaccuracy in the instrument, and half the remainder for impurity contracted subsequently to the experiment, we shall find 0·000138 as the increase in specific gravity to be corrected for.

In the commencement of this paper, I mentioned that the number 252·525 possessed the advantage of convenience; it is particularly obvious in calculating th [illegible] ghts of any determinate volume of a body, whose spe [illegible] avity in relation to water we have given. I shall just sta [illegible] xam [illegible] of the operation, as the rationale of the process will no doubt immediately strike the reader.

Suppose we have the specific gravity of brass to water as 8·3958 : 1, the weight of a cubic inch of brass will be found by setting down the specific gravity, with two cyphers annexed, three times successively under itself, each time writing the first figure of the line under the third of the preceding; then adding them together, thus:

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8395800
  8395800
    8395800
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84805975800
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the product is to be divided by 4, which leaves 21201493950 as the figures composing the answer; the common rules for pointing off decimals show us that the decimal places are to be seven; the answer will, therefore, be 2120·1493950 gr. It will also be perceived that from having the weight of a certain volume of any substance given, we may, by multiplying it by four, and employing a very easy mental operation, find the specific gravity.

In the synthesis of water, had the specific gravity of the gases and the weight of air been precisely correct, the atom of hydrogen would have come out 0·125; as it stands, the difference is very little.

The specific gravities here given, on the authority of Dr. Prout, are adopted by Meinecke in his stœchiometric table of gravities; indeed the specific gravity of oxygen seems very well established, being just the mean of those found by Saussure and Allen and Pepys.

Article III.

Preparation and Properties of Vestium, a newly discovered Metal. By Dr. Von Vest, Professor of Chemistry and Botany at the Johanneum, in Gratz.*

The nickel ore of Schladming, in Upper Steiermark, is mixed with cobalt pyrites. These pyrites, when we consider the ore with relation to the vestium, which it contains, must be considered as impurities, and, therefore, carefully separated. It is true indeed that the cobalt ore itself contains vestium; but the two metals are very difficultly separated from each other.

The nickel ore is to be pulverized and then fused. This fusion is necessary in order to separate the metals from all the earths, especially from lime, which otherwise would pass into the solutions; for the ore is very frequently mixed with calcareous spar. The pounded ore is mixed simply with pulverized glass, and put into a good air furnace, and exposed for an hour to a sufficient heat. A heat amounting to about 40° Wedgewood will be sufficient for the purpose.

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* Translated from Gilbert's Annalen der Physik, lix. 387; August, 1818.

The regulus is pulverized and digested on a sand-bath with nitric acid till all extrication of gas is at an end. The solution is renewed by the addition of a little muriatic acid. But as this acid dissolves a great deal of iron, and thus introduces that metal into the solution, it is better not to employ it. After the solution is decanted off, an additional dose of nitric acid is to be poured upon the residue, and the digestion renewed till a complete solution is obtained. The green solutions are now neutralized with carbonate of potash, and filtered. By this means most of the arseniate of iron separates as a white flocky precipitate.

Separation of a Part of the Arsenic.

The neutral solution is mixed with acetate of lead as long as any precipitate continues to fall, and the mixture is left standing for 24 hours in a warm place. The arseniate (and muriate) of lead falls to the bottom. In the heat, the acetic acid separates, and flies off, frequently with oxide of iron.

By this means the greater part, but by no means the whole, of the arsenic is separated. If the liquid after becoming clear is still rendered muddy by the solution of lead, a copious additional quantity of that solution is poured in, in order to remove the arsenic still more completely, and the mixture is allowed to remain till the whole of the precipitate collects at the bottom. The clear liquid is then drawn off, and the thick portion thrown on the filter. As there is usually an excess of lead in the liquid, I pour into it a portion of sulphuric acid, and separate the sulphate of lead formed by means of the filter.

Complete Separation of the Arsenic.

All the methods hitherto tried by me for separating the arsenic are only incomplete. But the following method seems to effect a complete separation.

I take a quantity of dry sulphuret of barytes still mixed with charcoal just as it is after its formation, by heating to redness sulphate of barytes and charcoal in a crucible, and put a portion of it, about as much as a couple of test-glasses will hold, into a large glass vessel. I then add a little water and as much diluted sulphuric acid as is just sufficient to neutralize the barytes. This is easily found by a few trials.

Into this mixture I pour with rapidity, before the sulphuretted hydrogen has had time to [illegible] escape, the green solution not yet quite free from arsen[illegible] stir [illegible] whole about with diligence to prevent the barytes from forming a powder. Instantly a great quantity of orpiment falls to the bottom.

This process I repeat till the whole of the arsenic is thrown down. After the separation of the orpiment, I try the clear liquid with water saturated with sulphuretted hydrogen gas. If it betrays arsenic, I repeat the preceding process till the sulphuretted hydrogen water ceases to indicate the presence of that metal. The liquid during the whole of this process should contain an excess of acid in order to prevent any other metal from being thrown down; though indeed a small loss of the other metals is scarcely to be avoided.

When the arsenic is thus removed and the liquid still acid, a small additional quantity of dry sulphuret of barytes may be put into it, by means of which the existence of sulphuretted hydrogen gas in the liquid may be continued for some time; but the whole must be frequently and carefully agitated to prevent the sulphate of barytes from cohering together in lumps. After some time, the clear liquid must be drawn off and the residue filtered, and the whole must be put into a wide vessel in a warm place and freely exposed to the air. The excess of sulphuretted hydrogen partly flies off, and is partly decomposed. We know that the process is completed when, some drops of the liquid being let fall into a potash solution, no black-coloured precipitate falls.

I now neutralize the solution with carbonate of potash, and digest it for some time in a warm place. This has a tendency to separate oxide of iron. This oxide and the sulphur are then separated from the liquid by the filter.

Method of freeing Vestium from Nickel.

I concentrate the clear solution obtained by the process above described till I bring it to a considerable consistence. A salt is formed and swims in the liquid in fine needles, like flakes of snow. I separate it by the filter, wash it with cold water, and evaporate the liquid a little further, in order to obtain an additional portion of the salt. I have obtained the same salt from some purified solutions of cobalt pyrites from the same mine. It is a salt of *vestium*.

I now dilute the green-coloured liquid with water, decompose it by potash, collect the precipitate upon a filter, wash it, and dissolve it in diluted sulphuric acid. In case there has been added an excess of acid, I saturate it with potash, then I add the requisite quantity of sulphate of potash, and evaporate the whole till it is reduced to the point of crystallizing.

The crust of salt obtained after the liquid has become cold I wash off with the requisite quantity of cold water, and separate the green-coloured and difficultly soluble nickel crystals from the white flocks lying on them, by agitation in a glass, and by gently rubbing them between the fingers, and I wash them very carefully, but without renewing the water. I then lay down the crystals to allow them to dry from a small residue of the solution before dissolving them again.

The ley in which the white flocks swim, and which is often rendered impure by oxide of iron, I collect in a glass, and after some little agitation decant it off from the oxide of iron at the bottom of the vessel; then mixing it with more sulphate of potash, I evaporate it again, to free it still more completely from all the nickel which it may contain. The crust of salt obtained is washed, as before described, with the requisite quantity of cold water. The cold ley contains sulphate of vestium, partly in solution and partly swimming in it in light flocks, often contaminated with iron, and very frequently with cobalt. When no more nickel crystals will separate, though the liquid has a green colour, it is a proof that too little sulphate of potash has been dissolved in it.

The green nickel crystals and the salt crusts obtained by the above described processes I mix with an additional quantity of sulphate of potash, pour water over the mixture, and set it in a warm place so that the salt may dissolve. I then allow the liquid to evaporate to dryness. The crystals of vestium which existed in the crusts, and which are thus separated, I remove from the nickel crystals by decantation. I treat the crystals and the liquid again and again in the same way till the nickel crystals assume a fine green colour. If it be our object to obtain pure nickel from them, it will be necessary to subject them to several additional crystallizations.

This is the way by which I separate all the nickel and all the uncombined oxide of iron. When the separation is completed, the solution is colourless, or nearly so. The vestium is now separated from it in the following manner:

I precipitate the liquid with carbonate of potash, which I boil with it, and then filter. The solution may be likewise evaporated to dryness, and heated to redness with carbonate of potash in a silver crucible. It may be then boiled and filtered. What remains on the filter, being sufficiently edulcorated, may be dissolved in nitric or muriatic acid. It is necessary to boil the vestium with the ley of carbonate of potash. This occasions no sensible loss of the vestium, though it be slightly soluble in cold potash. When the sulphate of vestium is fused with the potash, a sulphuret of vestium is often produced, when the ley happens to be polluted with charcoal. On that account, the boiling with the potash is preferable.

The vestium, after being heated to redness and well edulcorated, is digested cold in diluted muriatic acid, which scarcely acts upon it. The liquid is then poured off, and it is boiled in muriatic acid, which dissolves it readily. [illegible] vestium which has been boiled with the alkaline ley dis[illegible] easily in cold muriatic acid. We may likewise filter the [illegible] ley containing sulphate of vestium, boil the solid portion with an alkaline ley, and try the clear portion for vestium, after having treated it with caustic ammonia, to separate the iron, and filtered it. If it be green, it still contains some nickel. By decomposing it by potash and evaporating, we obtain the oxide of vestium.

Having by one or other of the methods just described obtained a solution of vestium in muriatic acid, I examine whether or not the solution be pure in the following manner. The impurities are owing to the presence of nickel, cobalt, and iron.

Nickel is known when a portion of the concentrated solution is precipitated by carbonate of potash, and the precipitate is digested in ammonia. If nickel be present, the ammonia assumes a blue or *green* colour; for a mixture of nickel and vestium colours ammonia green.

The solution still contains a great deal of nickel, which will be known by the blue colour; it must be decomposed by carbonate of potash, the precipitate dissolved in sulphuric acid, and the nickel must either be separated in crystals of sulphate of nickel-and-potash, or the whole being thrown down by carbonate of potash, some carbonate of ammonia is to be poured upon the precipitate; and after some agitation, the liquid is to be passed rapidly through the filter. By this means, the vestium will be left upon the filter mixed with only a very small proportion of nickel; and it may be still further purified by washing it with hot distilled water. The clear solution which passes through the filter contains most of the nickel; but it contains likewise some vestium, which gives it a green colour.

It is very difficult to free vestium from cobalt. On that account, it is advisable to free beforehand the ore employed from every perceptible portion of cobalt pyrites. A portion of the cobalt indeed may be separated by means of carbonate of ammonia in the same way as the nickel; or we may proceed in this way. We may dissolve the vestium containing cobalt in nitric, muriatic, or sulphuric acid, evaporate to dryness, and dissolve off the cobalt salt, which is much more soluble than the other; but by this process, we lose a portion of the vestium. But I am not at present acquainted with any better method of separating these two metals from each other.

The iron is detected in consequence of the blue colour which it strikes with prussiate of potash. The vestium may be freed from it by adding some nitric acid to the muriatic acid solution, heating to peroxidize the iron, and then throwing it down by means of a little caustic ammonia, and filtrating rapidly. But by this process, we lose a portion of vestium. I tried to separate the iron by means of prussiate of potash; but the filtered solution remained always either blue or green. It may be thrown down from neutral solutions by means of succinate of potash or a benzoate.

Some of the Properties of Vestium.

When vestium has been purified, it exhibits the following properties:

A.—*In its Salts.*

1. Oxide of vestium is soluble in sulphuric, muriatic, nitric, and acetic acids, and forms with them colourless solutions, having a metallic taste, and yielding by evaporation white crusts, or small needles like sulphate of lime. It separates from all the acids in white light flocks, when these crusts or needles are again dissolved in water, and allowed to remain at rest. These flocks are soluble in an additional quantity of acid, and when the liquid is heated.

In these properties, vestium agrees in part with some of the easily soluble metals; but the salts have the greatest resemblance, at least in the action of several reagents on them, with the salts of lime.

2. Prussiate of potash throws down vestium in milk-white flocks.

In this property, vestium agrees with several metals, besides the easily soluble ones. But the property distinguishes oxide of vestium from lime.

3. Sulphuretted hydrogen both in the liquid and gaseous state precipitates vestium of a dark *reddish-brown* colour. No precipitate falls if the solution or the reagent contains a slight excess of acid. When the precipitate is separated and in quantity, it appears black; but when floating in water, it has a brownish colour with a tint of red. The alkaline hydrosulphurets throw down vestium black.

By these properties, vestium shows its metallic nature and its difference from all other metals, except nickel and cobalt; for both these metals exhibit the same properties with sulphuretted hydrogen as vestium does. This is the reason why I could not, by precipitating with sulphuretted hydrogen, free vestium from these two metals; which I at first erroneously ascribed to their salts adhering to the precipitate.

M. Proust indeed has affirmed, and the statement is commonly believed, that nickel and cobalt are not precipitated from their solutions by sulphuretted hydrogen gas. But the assertion, if taken in all its generality, is not correct. The following seems to be a correct statement of the phenomena. Neither of the metals is precipitated from acid solutions by sulphuretted hydrogen gas; but both of them are precipitated by that gas from neutral solutions. But the precipitation in this last case soon reaches its limit; for the base being partially separated from the acid, this last begins to predominat [illegible] liquid; and when the acidity has advanced to a cer [illegible] t, all further precipitation is prevented.

The precipitation of vestium by sulphur [illegible] ydrogen no means a characteristic property of that metal, as I formerly imagined; but belongs equally to other metals with which vestium seems readily to associate. The precipitate of nickel and cobalt, by means of sulphuretted hydrogen, are easily distinguished from each other. The former is quite black, and swims about in a diluted solution, like fine soot, in soft particles. The precipitate in a diluted cobalt solution is reddish-brown, like that of vestium, and the solution resembles a strong infusion of coffee.

4. Pure ammonia precipitates vestium; but an excess of that liquid redissolves the precipitate, and the solution is colourless. The colourless appearance of this solution distinguishes vestium from nickel and cobalt. However, as by very great dilution we may obtain colourless solutions of both those metals, the proof is only of weight when we attend to the concentration of the liquids. If the vestium solution be very much concentrated, it assumes a yellowish tinge with ammonia; and if nickel be present, it assumes a greenish tinge; but it never becomes blue. I long ascribed this colour to the presence of iron; but I now believe that my opinion was erroneous, and that vestium itself, when very concentrated, has the property of tinging ammonia yellow.

5. Carbonate of ammonia throws down from muriate of vestium, even when it contains an excess of caustic ammonia, a snow-white powder. This powder does not become coloured, either by agitation or by longer digestion; nor does any sensible change take place in the liquid; but some oxide of vestium is dissolved which is only partially precipitated by rest or evaporation, but completely when carbonate of potash is added and the liquid evaporated.

Carbonate of ammonia scarcely occasions any precipitation in sulphate of vestium. Oxalate of ammonia precipitates the dissolved vestium from none of those solutions in carbonate of ammonia.

The phenomena exhibited by the sulphuric acid solution have some resemblance to those exhibited by lime; but the solubility of the oxide of vestium in carbonate of ammonia completely destroys the similarity. We see that vestium cannot be completely precipitated from its solutions by carbonate of ammonia, and not at all from its solution in sulphuric acid. Indeed, if we employ a very considerable quantity of the carbonate of ammonia, we may obtain a complete solution of the vestium; and solutions of vestium may in this way be examined.

6. Carbonates of potash and soda precipitate the vestium in the state of a carbonate; but the alkali retains a portion in solution, which is in part precipitated by boiling. If the ley be crystallized, crystals are frequently obtained containing vestium.

7. Lime-water precipitates the vestium in light-white flocks, provided there be no ammonia in the solution. Sulphuretted

ydrogen, water being now added, occasions no further alteration n the liquid.

8. Caustic alkalies likewise precipitate vestium, and sulphuretted hydrogen scarcely colours the precipitate; but if an acid be poured in, the colour becomes brown.

9. A solution of common pure borax occasions no precipitate in diluted vestium solution; but sulphuretted hydrogen throws down the vestium in that case likewise.

As borax contains an excess of soda, I did not expect this result, and it is difficult to account for it. Probably the degree of concentration of the solution has an influence upon it.

10. Tincture of nutgall throws down from the sulphuric and muriatic solutions of vestium a very small quantity of a white salt. From diluted nitric acid solution, it throws down nothing.

11. Oxalate of potash throws down a copious white precipitate.

12. The succinates trouble the solutions of vestium only feebly.

13. The precipitate of vestium by sulphuretted hydrogen dissolves with effervescence in nitric acid.

14. Phosphate of soda throws down vestium white.

15. A plate of zinc left for some days in a solution of vestium throws down white flocks.

I conceive that the experiments here stated are sufficient to demonstrate, that a peculiar metallic substance exists in the solutions. The colourless solutions, the white salts, their solubility in water, the relation of these solutions to sulphuretted hydrogen and to ammonia, the white precipitates, the being thrown down by zinc—all these properties characterize this body as a peculiar metal differing from every other hitherto known.

B.—*Oxides.*

By rest and evaporation vestium separates itself from solutions, as we have seen, in white flocks, similar in appearance to fine mashed paper. Acids do not dissolve these flocks so easily as they do newly precipitated carbonate of vestium. I conclude from this, that vestium combines with two different doses of oxygen; for in the preceding respect, its properties resemble those of salts of iron and tin.

It deserves attention that the oxides are distinguishable according to the medium in which they are formed. Lead, mercury, cobalt, and nickel, when they unite with acids, have obviously a different degree of oxidizement from that which exists in the oxides formed in the air, or when they unite with alkalies. The bodies with which they unite undoubtedly alter the proportion of their oxygen. The oxides of [illegible] and lead are in salts undisputably white, and not coloure[illegible] the case with their oxides formed in the air. Cobalt [illegible]ve a oxide, as it gives that colour to acids, and this [illegible] know, peculiar to it; but the oxide of cobalt formed in the air is not red. I believe that the oxides of metals formed in the air, in acids, and in alkalies, may be distinguished from each other.

All the oxides of vestium are white, as is the case with the oxides of antimony and zinc. Their colour is not altered by exposing them to a red heat, when they are quite free from mixture. A mixture of cobalt or nickel makes them blackish.

Borax is not coloured by oxide of vestium; but becomes often dull and opaque; because it dissolves the oxide of this metal with difficulty. The oxide of vestium when fused with saltpetre remains white. After being strongly heated to redness, it is with difficulty affected by acids.

C.—*Reduction to the metallic State.*

I have attempted in vain to reduce oxide of vestium by simply mixing it with charcoal and heating; though I exposed it to the temperature successively of 50°, 100°, and 140°, of Wedgewood's pyrometer. The oxide came out of the furnace simply agglutinated, and had the appearance of pumice.

I then added a mixture of borax, porcelain, earth, and quartz, to assist as a flux, and exposed the oxide to a heat of 120° Wedgewood. The flux united with the oxide, and formed an opaque milk-white glass.

But when oxide of vestium is mixed with arsenic, it is reduced at a moderate temperature. The small regulus had the appearance of iron, was brittle, and had a fine granular texture. When dissolved in nitric acid, and freed from arsenic and all other metals with which it was mixed, it gave the white salt which I had previously obtained from the ore.

The peculiar ore of vestium is unknown to me. In the nickel ore of Schladming, which is of a very compound nature, it seems to be only mechanically mixed; for the proportion of it obtained in different trials was very different.

Some additional Experiments on the Reduction of Vestium.

I took the white flocks separated by agitation from the nickel crystals, and digested them for 24 hours in carbonate of potash. Though by this process I by no means succeeded in separating the whole of the sulphuric acid, I made the following experiment with the white matter that remained. After filtration and edulcoration, I put the flocks into muriatic acid, and poured into the liquid an excess of carbonate of ammonia, to remove any nickel which might be present. I then triturated the white mass with half its weight of oxide of arsenic and a little charcoal, put the mixture into a charcoal crucible, which I enclosed in a hessian crucible surrounded with charcoal powder, and exposed it for an hour to a heat between 60° and 70° Wedgewood. A part only of the mass was reduced to a regulus, which had undergone complete fusion. The remainder formed a whitish, very hard, vesicular body with an earthy fracture, which was a fused mixture of sulphuret of potash and oxide of vestium.

The regulus, being dissolved in nitric acid, and evaporated to dryness, gave a yellowish powder. This powder being boiled in muriatic acid dissolved with difficulty, and formed a yellow-coloured liquid, which contained iron, although I had not observed the presence of that metal before the reduction. The liquid was precipitated by carbonate of ammonia; along with the iron there fell a good deal of white, slimy carbonate of vestium. The ammonia was bluish. Vestium, over which nitric acid is boiled to dryness, becomes with difficulty soluble in acids, and seems to be converted into another oxide.

A portion of the regulus, which I dissolved in nitro-muriatic acid and precipitated by caustic ammonia, gave me a rose red solution, and oxide of iron remained on the filter. When the red solution was evaporated, there remained a white residuum coloured by cobalt, which was not again completely soluble, but left a white matter tinged slightly red by cobalt. This residue being separated from the solution, it was dissolved in muriatic acid, evaporated till white flocks fell, which were separated from the cobalt solution by decantation. These white flocks became brown when treated with sulphuretted hydrogen water. The oxide of vestium could not be precipitated from the ammoniacal solution by carbonate of potash; probably because the excess of ammonia prevented the precipitate from appearing.

I boiled the white flocks, separated from the green nickel crystals by decantation, in muriatic acid, filtered, and decomposed the clear solution by means of caustic ammonia. The ammoniacal solution was greenish. Carbonate of potash being dropped into it, a white precipitate fell, which I collected on a filter, and washed. I evaporated the ammoniacal solution, poured sulphuric acid into it, and set it aside to crystallize, in order to obtain the oxide of vestium which it still contained.

The precipitate on the filter, when dried, was a fine white powder with a shade of blue. It, therefore, contained cobalt which had been precipitated from the ammoniacal solution by the potash. I rubbed this powder with an equal volume of white arsenic, and with four times as much black flux, put it into a crucible, and exposed it to the temperature of 70° Wedgewood for an hour. I obtained a metallic button. On dissolving this button in nitro-muriatic acid and evaporating the solution, there remained a reddish crust of arseniate of cobalt. I softened it with water, digested it for some hours in muriatic acid, and then washed it out. There now remained behind a white gelatinous mass, which, being fused with borax, communicated no colour.* It thus appears that there was in the regulus, besides the cobalt, another substance, which formed white flocks, and did not communicate any colour to borax (therefore vestium).

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* When strongly concen[illegible] gelatinizes in acid solutions. It must be dried by exposure to heat.

I dissolved a portion of the impure regulus, first obtained from the ore in nitric acid, and without freeing it from the arsenic, I precipitated by potash. The precipitate was dissolved in sulphuric acid, sulphate of potash was added, and the liquid was brought to the degree of concentration requisite to yield crystals. The nickel crystals were at first, as was usually the case, very light green; but by repeated solutions and crystallizations, they became darker and darker, because more of the white oxide of vestium was separated every time it was crystallized. The white flocks obtained by all these processes I mixed with sulphate of potash, and again evaporated to separate the nickel still more completely. I then digested it with sulphate of potash, filtered, and reduced the oxide in a good wind furnace with common salt and charcoal. The regulus which I obtained had a very fine granular fracture, was very brittle, and its fracture showed a very white colour. By exposure to the air, it soon lost its lustre.

I dissolved a portion of this regulus in nitro-muriatic acid, and decomposed a portion of this green solution (for nickel has a very strong colouring power upon acids) by potash. I dissolved the white precipitate in sulphuric acid, added a portion of sulphate of potash, in order to separate the nickel by crystallization, and washed the salt formed in cold water. The white flocks, which rendered the solution muddy, were separated, and decomposed by carbonate of ammonia added in excess. By this means I obtained a fine white precipitate; and the ammonia was but slightly coloured. Thus I procured vestium merely in combination with arsenic (for that metal had not been separated).*

The iron is sometimes of very difficult separation, sometimes it is easily got rid of. This diversity seems to depend upon the different portions of iron contained in the ore under examination.

One portion of the vestium may be easily purified, by dissolving the carbonate in nitro-muriatic acid, evaporating to dryness, and washing the dry mass with water. The dry residue (submuriate of vestium) is tolerably pure, or at least may be made so by a second evaporation with muriatic acid. But a great deal of vestium remains in the solution along with iron, nickel, and cobalt. These experiments are not always equally successful. The solution may contain arsenic or not: when the arsenic is entirely removed, the oxide of vestium cannot be reduced to the metallic state.

I tried to decompose by carbonate of potash a solution of the impure regulus, after I had mixed it with sal ammoniac. The solution was at first blue from the nickel which it contained; but it soon changed into green. A white precipitate fell, which resembled vestium. The green ammoniacal ley being neutralized with sulphuric acid and evaporated, let fall, besides nickel crystals, flocks of vestium. As these appeared after the solution had stood some days, I conceive it to be the vestium and not the oxide of iron which changes the blue colour of an ammoniacal solution of nickel into green; for the iron separates itself much sooner.

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* Another time I precipitated the solution by acetate of lead, expecting in vain to separate the arsenic by that way and get the vestium pure.

6. *Method of determining the Presence of Vestium in Ores.*

We can determine the presence of vestium in ores three different ways.

1. The ore fused into a regulus is to be dissolved in nitromuriatic acid, and the arsenic separated by the process above described. Decompose the solution freed from arsenic by caustic ammonia not in a state of too great concentration, and after filtering, add to the liquid carbonate of potash. If vestium be present, it separates altogether in the state of a white precipitate.

2. Or the ammoniacal solution may be decomposed by oxalate of potash, which throws down the vestium. If, on the other hand, carbonate of ammonia be poured into the solution, the vestium is not precipitated.

3. Or without freeing the muriatic solution from arsenic, we may pass a stream of sulphuretted hydrogen gas through it as long as a precipitate continues to fall. This precipitate being collected and heated is very easily reduced into the metallic state. Dissolve the regulus in nitric acid, and treat the solution in the way above described; or the metal may be thrown down by carbonate of potash, the precipitate be redissolved in sulphuric acid, decomposed by ammonia or potash, and evaporated. In short, we must proceed as in the preparation of vestium above described.

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Appendix by the Editor.—It is evident from the preceding paper, that Dr. Vest has never obtained his new metal free from arsenic, nickel, and cobalt. Hence the experiments of Mr. Faraday and Dr. Wollaston (Royal Institution Journal, vi. 112) cannot be considered as sufficient to invalidate the existence of the substance called vestium by Dr. Vest. If his account of that substance be accurate, of which it will not be in our power to judge till the nickel ore of Schladming is examined by some other person, it is obviously different from every other metal with which we are at present acquainted.

ARTICLE IV.

On the Anthrazothion of Von Grotthuss, and on Sulphuretted Chyazic Acid. By R. PORRETT, Jun.

THE new name which M. Grotthuss has given to this acid, he informs us, was in consequence of his discovering that the name of sulphuretted prussic acid, which in Germany had been substituted as a synonyme for sulphuretted chyazic acid, was not applicable; as he had ascertained by his experiments that although it contained the same elements as prussic acid, yet they did not exist in it in the same proportions, and that neither prussic acid nor cyanogen as such exist in it. I trust, however, to be able to prove to chemists that this assertion is exceedingly erroneous; and that consequently no such reason exists for adopting the new term recommended by M. Grotthuss.

Besides new naming a substance already known, M. Grotthuss has given the name of anthrazothion to a principle which is not known, but which he conceives to exist in some sulphuretted chyazates, although he acknowledges that he has not been able to isolate it. It will be time enough to consider the propriety of this name when the principle itself has been obtained; in the mean time it will be useful to look a little into the arguments and suppositions on which its existence is defended.

In examining the suppositions which M. Grotthuss makes in order to admit the existence of anthrazothion, it appears that having concluded the sulphuretted chyazate of protoxide of copper to be a compound of anthrazothion with metallic copper, he explains its formation when sulphuretted chyazate of potash is poured into a mixed solution of a salt of peroxide of copper with a disoxidizing body, by supposing that the disoxygenating substance combines with $\frac{1}{5}$th of the oxygen of the peroxide, and that the remaining $\frac{4}{5}$ths combine with and separate the hydrogen from the sulphuretted chyazic acid, converting it to anthrazothion, which unites to the reduced copper; he further supposes that the water formed from the oxygen of the copper with the hydrogen of the acid enters into the new compound, which he consequently terms an anthrazothionhydrate.

A very simple experiment will suffice to show the fallacy of these suppositions. Let the sulphuretted chyazate of protoxide of copper be decomposed by a solution of potash, sulphuretted chyazate of potash will then be formed, and *protoxide of copper will remain;* now if the decomposed salt had been a compound of anthrazothion and copper, the water present must have furnished hydrogen to the anthrazothion, and oxygen to the copper; but as, according to M. Grotthuss, anthrazothion combines with three atoms of hydrogen, it must detach an equal number of atoms of oxygen from the water; two of these atoms of oxygen would combine with the metal and convert it to *peroxide,* and the other atom must escape as gas; but as in the process no peroxide is formed, nor any oxygen gas liberated, it is very clear that the compound cannot be such as M. Grotthuss conceives it to be.

M. Grotthuss asserts that this compound "contains a notable quantity of water, though Porrett affirms the contrary:" this quantity he afterwards states at $\frac{1}{8}$th of its weight. I did certainly affirm the contrary; and having since repeated my former experiments, I now reaffirm it; however, I by no means intend to assert that no water can be formed when it is heated so highly as to be decomposed; for as, according to my experiments, its acid contains an atom of hydrogen, and its oxide an atom of oxygen, it follows that an atom of water should in that case be produced which did not pre-exist as water in it: the weight of this water, however, would not exceed $\frac{1}{15}$th of the sulphuretted chyazate, and would only amount to about half the quantity which M. Grotthuss procured. I can only account for the remaining half on the supposition that he had not sufficiently freed by lixiviation the sulphuretted chyazate which he employed from adherent salts which would surrender their water of crystallization when heated.

The arguments used by Von Grotthuss to induce a belief that the copper exists in this compound in the metallic state are the following:

1. That by the action of heat upon it, a peculiar gaseous body separates with a particular smell, which M. Grotthuss, both from the analogy of cyanogen and because it is absorbed by ammonia, and then strikes a blood-red colour with solutions of iron, considers as anthrazothion.

2. That after the action of heat there remains a sulphuret which contains the copper in the metallic state.

3. That it is nearly insoluble in muriatic acid, whilst, on the contrary, the alkaline sulphuretted chyazates are very soluble therein.

4. That during the combination of sulphuretted chyazic acid with easily reducible oxides, the former must undoubtedly reduce the latter, because its carbon, sulphur, and hydrogen, are each capable of reducing such oxides.

5. That at the instant when it is forming in a mixture of acetate of copper and alcohol, a brown colour is perceptible, which disappears when it is completely formed.

The insufficiency of these arguments for the purpose for which they are advanced will appear from the following observations:

1. What M. Grotthuss conceives to be a peculiar gaseous body, and which he con[illegible]s anthrazothion, I have found to be only a mixture of ga[illegible]pours, principally consisting of a compound of sulphur [illegible]ogen, [illegible]bling that formed

when cyanogen and sulphuretted hydrogen are mixed together; it also contains azote and sulphuret of carbon, the latter readily distinguishable by its peculiar smell, and sometimes a minute quantity of sulphuretted chyazic acid comes over with it unaltered. From 10 gr. of sulphuretted chyazate of copper, I obtained about two cubic inches of this mixture of gas and vapour over mercury; but this diminished considerably in bulk as the vapour condensed, the surface of the mercury at the same time becoming tarnished; and there remained after the action of an alkaline solution only about half a cubic inch of azote. The same quantity of sulphuretted chyazate, when previously mixed with half its weight of copper (in that state of minute division in which it is precipitated from its solutions by iron) gave, on the application of heat, about three cubic inches of permanent gas, which was cyanogen.

2. The sulphuret left behind containing the metal in the metallic state, is nothing more than what should occur considering that the oxygen in the protoxide is only in the proportion necessary to form water with the hydrogen of the acid; it therefore follows, as I said before, that when the compound is decomposed by heat, water must be formed, and the metal reduced; but it does not follow that the metal was in this state before that decomposition takes place.

3. The little solubility of sulphuretted chyazate of copper in muriatic acid will, I apprehend, be thought a very inadequate proof that it contains the copper in the state of metal, when it is considered that oxalate of copper has also very little solubility in this menstruum, and yet it contains the copper in the state of peroxide; and that, on the other hand, cyanuret of mercury, which contains the metal in the metallic state, is very soluble therein.

4. With respect to the argument that the carbon, hydrogen, &c. of the sulphuretted chyazic acid must undoubtedly reduce the easily reducible oxides, I conceive it will be sufficient answer to state that, if there were any truth in this remark, the acetates of silver, mercury, and copper, with a variety of similar salts, could have no existence.

5. With regard to the brown copper colour which a mixture of acetate of copper with alcohol momentarily assumes when sulphuretted chyazate of potash is added to it, I can assert that the mixture becomes indeed brown, but presents no metallic appearance; the colour is exactly similar to that of protomuriate of copper when it contains a little permuriate; it, therefore, merely indicates the presence of protoxide in the solution, but not that of metallic copper.

Having thus shown that no grounds exist for considering sulphuretted chyazate of copper as an anthrazothionhydrate, I shall now show its real composition.

In an experiment detailed in my paper, on the nature of the triple prussiates, in the Philosophical Transactions for 1814, I stated that from five gr. of sulphuretted chyazate of copper, decomposed by nitric acid, I had obtained, by means of iron, 2·82 gr. of metallic copper, which, reckoned as protoxide, is equal to 3·173 gr. or to 63·44 per cent.

I have since found that this determination was not quite accurate, the quantity of copper having been overstated by about $\frac{1}{3}$d of a grain; the true quantity is 2·45 gr. equal to 2·75 of protoxide, or to 55 per cent. Therefore sulphuretted chyazate of protoxide of copper is composed of

Acid	45	73·94 = 1 atom
Base	55	90·00 = 1 atom
	100	163·94 = 1 atom

In the same paper it will be seen, that by acidifying the sulphur in five gr. of the above compound, and adding a salt of barytes, I had obtained 8·86 gr. of sulphate of barytes representing 1·20 gr. of sulphur; on repeating this experiment with all imaginable care, I have obtained about $\frac{1}{7}$th of a grain more, the quantity being 9 gr. equal to 1·22 sulphur. Von Grotthuss, who also repeated this experiment, obtained only 8·1 gr.; but he has certainly underrated it.

Now as 5 gr. of this compound contain 1·22 gr. of sulphur, 100 must contain 24·4, and this deducted from 45, leaves 20·6 gr. as the weight of the other constituents of the acid in 100 gr. of the sulphuretted chyazate. These other constituents I have long since stated to be the elements of prussic acid in the proportions in which they exist in that acid; and notwithstanding Von Grotthuss's denial of this statement, I must be allowed to persist in it; an experiment which I shall presently describe will fully justify me in so doing.

We have it now in our power to state the constituents of sulphuretted chyazate of copper more in detail, as follows:

Sulphur	24·4	40·00 = 2 atoms
Carbon, azote, and hydrogen, in the proportions which form prussic acid	20·6	33·94 = 1 atom of prussic acid
Sulphuretted chyazic acid	45·0	73·94 = 1 atom
Protoxide of copper	55·0	90·00 = 1 atom
	100·0	163·94 = 1 atom

But in order to put the composition of this salt and of its acid out of all doubt, I undertook the analysis of it by combustion with peroxide of copper [illegible] purpose I mixed 4·8 gr. of it intimately by trituration [illegible] r. of [illegible] peroxide, the mixture was put into a glass tube [illegible] a small retort, and was heated by the flame of a spirit lamp, the gas produced was received over mercury, it measured 6·34 cubic inches at mean temperature and pressure, and on analysis I found it composed as follows:

	Cubic inches.	Volumes.	Grains.	
Carbonic acid..	3·46	2	1·611	0·44 carbon 1·17 oxygen
Azote.........	1·73	1	0·513	
Sulphurous acid	1·15	⅔	0·781	0·39 sulphur 0·39 oxygen
	6·34			

The residuum in the glass tube was a mixture of sulphuret of copper, protoxide of copper, and metallic copper; it weighed 13·56 gr. I heated it in diluted sulphuric acid, which decomposed the protoxide, reduced half of it to the metallic state, and converted the remaining half into peroxide, which it dissolved. The copper in the metallic state was dissolved by weak nitric acid in the cold, and the sulphuret remained unacted upon: by these means I ascertained that the residuum was composed of the following ingredients:

	Grains.	
Protoxide of copper	7·56	0·84 oxygen 6·72 copper
Copper....................	2·10	
Sulphuret of copper	3·90	0·78 sulphur 3·12 copper
	13·56	

Previously to drawing the proper inferences from these data, it will be convenient to estimate the quantity of oxygen consumed, and also that contained in the produced gases.

	Oxygen grains.
Contained in the 12 gr. of peroxide	2·400
Contained in the protoxide of the 4·8 gr. of sulphuretted chyazate..	0·293
First introduced in the tube.........................	2·693
Remained in the tube in 7·56 gr. of protoxide..........	0·840
Total oxygen consumed.............................	1·853
In 1·611 gr. of carbonic acid.........................	1·170
In 0·781 gr. of sulphurous acid......................	0·390
Total oxygen expended in the formation of gas.........	1·560

Deducting 1·560 from 1·853, it appears that 0·293 of a grain of oxygen (equal to ¼ of that in the carbonic acid) was consumed beyond what was expended in the production of gases; this must have formed water with the hydrogen of the sulphuretted chyazic acid, and represents 0·038 of a grain of hydrogen.

The constituents of the 4·8 gr. of sulphuretted chyazate may now be easily ascertained in the following manner:

	Grains.		Grains.	
Sulphuretted chyazic acid.	2·162	Sulphur. .	1·172	Equal to that in the sulphurous acid and in the sulphuret of copper, or 0·39 + 0·78.
		Carbon . .	0·439	Equal to that in the carbonic acid gas.
		Azote . . .	0·513	Equal to that in the azote gas.
		Hydrogen	0·038	Equal to that which combined with 0·293 oxygen.
Protoxide of copper. . . .	2·638	Copper . .	2·345	Equal to that contained in the residuum minus what was contained in the peroxide first introduced, or 11·94 — 9·6.
		Oxygen. .	0·293	Equal to the whole oxygen consumed, + that left in the residuum, — that first introduced as a constituent of the peroxide, or 1·853 + 0·84 — 2·4.
	4·800		4·800	

These results, when stated in atomic proportions, give the following numbers, which completely confirm the former analysis.

One atom of sulphuretted chyazic acid . . .	73·94	Sulphur	40·00	Two atoms
		Carbon	15·08	Two atoms
		Azote	17·54	One atom
		Hydrogen . .	1·32	One atom
One atom of protoxide of copper	90·00	Copper.	80·00	One atom
		Oxygen	10·00	One atom
One atom of sulphuretted chyazate of copper	163·94		163·94	

It deserves to be remarked in this experiment, that had the whole of the sulphur been converted into sulphurous acid, its volume would have exactly equalled that of the carbonic acid, and that a concise statement of the whole phenomena of its perfect combustion may be made by saying, that an atom of sulphuretted chyazic acid in combining with nine atoms of oxygen forms two atoms of sulphurous acid, two atoms of carbonic acid, one atom of water, and relinquishes one atom of azote.

The formation of ammonia in liquid sulphuretted chyazic acid by the action of concentrated acids, and considered by Von Grotthuss as proving that the azote and hydrogen exist in it in the ratio of one to three, proves no such thing. I have often observed this formation; it never takes place without the simultaneous production of carbonic or sulphurous acid: hence in these instances water is decomposed which supplies the additional hydrogen requisite to form ammonia with the azote and hydrogen of the acid, and gives up a corresponding proportion of oxygen to the carbon or sulphur.

I felt much surprise at Von Grotthuss's confident denial of the production of prussic acid from sulphuretted chyazic acid, which I had asserted to take place when its sulphur is acidified. This production is so abundant not only in the cases in which I described it as occurring, but in a variety of others, as M. Vogel has since proved, that it is quite extraordinary that M. Grotthuss did not perceive it.

But as one mistake requires others to support it, so I find M. Grotthuss asserting, that "as Porrett did obtain prussic acid, there can be no doubt that he operated on a salt containing a prussiate mixed with it, which would be more readily the case as he employed no alcohol, nor indeed any method whatever, to separate sulphuretted chyazate of potash from the prussiates." Now it so happens, that it was by the employment of alcohol that I first obtained the salt in question, as may be seen by reference to the paper in which I announced its discovery, and which was published in the Transactions of the Society of Arts for 1809; and as to my not employing any other method of separating the prussiates, I beg to refer chemists to the following directions, which form a part of the process published in the Philosophical Transactions for 1814; and the object of which was to separate the prussic from the sulphuretted chyazic acid by taking advantage of the greater volatility of the former:

"Let the clear liquor (containing the two acids united to potash) be brought to a decidedly acid state by the addition of sulphuric acid; then keep it for a short time at nearly the boiling point."

It is unnecessary to point out any further inaccuracies of M. Grotthuss on this subject, especially as some of them have been already noticed in M. Vogel's paper on sulphuretted chyazic acid. This chemist has besides given an improvement on Von Grotthuss's process for obtaining sulphuretted chyazate of potash. I have repeated the process in the mode recommended by M. Vogel, which I find to answer the purpose exceedingly well; it appears, however, to be still susceptible of advantageous modifications: one of these I consider to be a reduction of one quarter the quantity of sulphur which he employed; the other consists in first expelling by a moderate heat all the water of crystallization from the ferruretted chyazate; the sulphur then combines quietly with the salt immediately on fusion with it; and with these proportions there is no great excess of sulphur remaining. I have endeavoured to acquire some idea of what passes in this operation, and have ascertained that 10 gr. of ferruretted chyazate of potash combine with 4 gr. of sulphur, and give off 1·35 gr. of water, and 0·5 of a grain of mixed gas and vapour, measuring at mean temperature and pressure 0·3 of a cubic inch, half of which was absorbable by water, to which it gave the smell of sulphuret of carbon, and the rest may have been carburetted hydrogen; but an accident prevented any particular examination of it. The residuum weighed 12·15 gr.; it was composed of sulphuretted chyazate of potash, of sulphuret of iron, and of a solid compound of charcoal with sulphur: hence it appears probable, that in this experiment the ferruretted chyazic acid, which I have before shown to be a compound of four atoms carbon, one of azote, two of hydrogen, and one of iron (*Annals of Philosophy*, vol. xii. 1818), relinquishes half its carbon and hydrogen, with the whole of its iron, and takes two atoms of sulphur in exchange, the liberated carbon and iron at the same time combining with sulphur.

I beg to add to this communication, as being closely connected with the subject, the results of a slight and not very precise examination, which I made some time since, of the yellow crystals, which Gay-Lussac discovered were formed when cyanogen and sulphuretted hydrogen gases are mixed together over mercury: I have remarked that these crystals are not formed when the two gases are quite dry, but that they are quickly produced if a drop of water is passed up into the mixture. The colour and the nature of the crystals obtained do not appear to me to be always the same; they are sometimes greenish-yellow, and soluble in water; at other times they are orange-brown, and only partially soluble therein: I believe that the former are produced when the cyanogen employed is only ⅔ds the volume of the sulphuretted hydrogen; and that the latter make their appearance when the volume of cyanogen exceeds that of the sulphuretted hydrogen; however this may be, the aqueous solutions, after the separation of the brown deposit from the orange-coloured crystals, appear identical in their chemical characters. This solution possesses none of the properties of sulphuretted chyazic acid; it does not change the colour of litmus; it has no effect on solutions of iron, nor on other metallic solutions, with the exception of those of gold, silver, palladium, and mercury, in which it produces brown and grey precipitates; it contains neither prussic nor sulphur chyazic acid; yet this latter acid is formed in it when it mingled with an alkali and then with an acid. The same treatment does not form any prussic acid.

In conclusion, I take the opportunity of recording a few observations which I have made on the action of iodine, of hydriodic acid, and of sulphuretted hydrogen, on prussic acid, on cyanuret of mercury, and on sulphuretted chyazate of copper.

Iodine decomposes the aqueous solution of prussic acid, and becomes hydriodic acid, cyanogen being at the same time evolved.

On the contrary, hydriodic acid is itself decomposed by cyanuret of mercury, red ioduret of mercury and prussic acid being formed. The affinity of mercury for iodine doubtless determines this decomposition.

Iodine, when put into a solution of cyanuret of mercury, sets the cyanogen at liberty, and forms red ioduret with the metal.

Sulphuretted hydrogen gas, when quite dry, does not appear to act on sulphuretted chyazate of copper; but it instantly decomposes it when water is present, sulphuretted chyazic acid being separated, and sulphuret of copper formed.

Tower, April 3, 1819. R. Porrett, Jun.

Article V.

New Demonstrations of the Binomial Theorem. By Mr. Herapath.

(To Dr. Thomson.)

Among the many demonstrations that have been given of the binomial theorem, I do not remember to have seen one that is both elementary and complete. That in the Calcul des Fonctions is, perhaps, one of the most elegant and complete that has yet been given; but it has been objected to as not being elementary. The same objection might, with a little modification, be made to one or two neat demonstrations that have appeared in some of the late volumes of the Philosophical Transactions, and to others that I have met with in different authors. It seems that mathematicians have considered the lower branches of algebra to be quite insufficient, without some assistance from the higher analysis, to effect a proof of this celebrated theorem. Whether Newton's not attempting to demonstrate this, one of the most beautiful and valuable of his mathematical discoveries, and his resting satisfied of its general truth merely from trials in a few particular cases, may have had any influence, I will not take upon me to determine; but I hope the following demonstration, drawn from common algebra, will show, that there is no necessity of having recourse to other principles to obtain a proof simple, direct, and complete. If it has any other merit, it is to the best of my knowledge that of novelty.

Knowle-hill House. J. HERAPATH.

Of the Invention of a Theorem for raising a Binomial to any whole positive Power.

Let $a + b$ be any binomial, then if we take the successive powers of it by actual multiplication, there will result,

Power.			
1.	$a + b$	or the coefficients alone will be	1, 1.
2.	$a^2 + 2ab + b^2$		1, 2, 1.
3.	$a^3 + 3a^2b + 3ab^2 + b^3$		1, 3, 3, 1.
4.	$a^4 + 4a^3b + 6a^2b^2 + 4ab^3 + b^4$		1, 4, 6, 4, 1.
5.	$a^5 + 5a^4b + 10a^3b^2 + 10a^2b^3 + 5ab^4 + b^5$		1, 5, 10, 10, 5, 1.
6.	$a^6 + 6a^5b + 15a^4b^2 + 20a^3b^3 + 15a^2b^4 + 6ab^5 + b^6$		1, 6, 15, 20, 15, 6, 1.

from which it is manifest, as well as from a consideration of the process of multiplication by which those coefficients are produced, that the coefficient of any term is equal to the sum of the coefficients of the corresponding and preceding terms of the next lower power; as, for instance, in the sixth power, $1 = 1 + 0$, $6 = 5 + 1$, $15 = 10 + 5$, &c. Therefore the coefficient of the second term of any whole positive power n, must be n; that of the first term being unity.

Again, if we divide the coefficients of the third terms by those of the second, we shall have

Coefficients of second terms or exponents of the powers....	$1, 2, 3, 4, 5, 6, 7 \ldots\ldots\ldots n$
Quotients of third by second coefficients..................	$\frac{1}{2}, \frac{2}{2}, \frac{3}{2}, \frac{4}{2}, \frac{5}{2}, \frac{6}{2} \ldots\ldots\ldots \frac{n-1}{2}$

Whence $\frac{n-1}{2}$ is the general multiplicator by which the third coefficient is produced from the second. Consequently the power being n, the coefficient of the third term is $n \times \frac{n-1}{2}$.

And generally, by following the same method of dividing the succeeding by the preceding coefficients, we shall have

Indices or coeff. of 2d terms	$1, 2, 3, 4, 5, 6, 7, 8, 9 \ldots n$ or $\frac{n-0}{1}$
Quotients of 3d by 2d coeff.	$\frac{1}{2}, \frac{2}{2}, \frac{3}{2}, \frac{4}{2}, \frac{5}{2}, \frac{6}{2}, \frac{7}{2}, \frac{8}{2} \ldots\ldots \frac{n-1}{2}$
4th by 3d coeff.	$\frac{1}{3}, \frac{2}{3}, \frac{3}{3}, \frac{4}{3}, \frac{5}{3}, \frac{6}{3}, \frac{7}{3} \ldots\ldots \frac{n-2}{3}$
5th by 4th coeff.	$\frac{1}{4}, \frac{2}{4}, \frac{3}{4}, \frac{4}{4}, \frac{5}{4}, \frac{6}{4} \ldots\ldots \frac{n-3}{4}$
mth by $(m-$	$\frac{1}{m-1}, \frac{2}{m-1}, \frac{3}{m-1}, \ldots \frac{n-(m-2)}{m-1}$ o

$\frac{n-m+2}{m-1}$.

Therefore it is evident that the multiplicators by which the succeeding coefficients are generated out of the preceding are the terms of this series $\frac{n-0}{1}, \frac{n-1}{2}, \frac{n-2}{3}, \frac{n-3}{4} \ldots\ldots \frac{1}{n-2}$; and consequently the general theorem for raising a binomial to any whole positive power is $a^n + n\,a^{n-1}\,b + n \,.\, \frac{n-1}{2}\, a^{n-1}\, b^2 + n\, \frac{n-1}{2} \,.\, \frac{n-2}{3} a^{n-3} b^3 \ldots\ldots n \frac{n-1}{2}, \frac{n-2}{3} \ldots\ldots \frac{1}{n-2}\, b^n$. I do not say any thing of the law of the exponents of a, b, because it is too obvious to require elucidation.

In this way would the invention of the theorem have been much more natural and easy, at least for whole positive powers, than the method of interpolations followed by Newton; and it being obtained for this case, the others are easily deduced from it. Thus for the case of

Whole Negative Powers,

We have $A = (a+b)^{-n} = \frac{1}{(a+b)^n} =$

$$\frac{1}{a^n + n\,a^{n-1}\,b + n\,\frac{n-1}{2}\,a^{n-2}\,b^2 + \&c.} = \text{by common division}$$

$$a^{-n} - n\,a^{-n-1}\,b - n\,\frac{-n-1}{2}\,a^{-n-2}\,b^2 - n\,\frac{-n-1}{2}\,.\,\frac{-n-2}{3}\,a^{-n-3}$$

$b^3 - \ldots\ldots$ &c. which shows that the general theorem for positive exponents, being whole numbers, is equally true for negative.

The same principle discovers the form of the theorem when the exponent of the power is *fractional*.

For if the terms of the fraction be m, n, and we have $x + y = (a+b)^{\frac{m}{n}}$; by involution we get $(x+y)^n = (a+b)^m$; and therefore $x + y = \frac{(a+b)^m}{(x+y)^{n-1}} = (a+b)^m \times (x+y)^{\overline{-n-1}}$

$$= a^m \left(\frac{1}{x^{n-1}} - \overline{n-1}\,\frac{y}{x^n} + \overline{n-1}\,.\,\frac{n}{2}\,\frac{y^2}{x^{n+1}} - \overline{n-1}\,.\,\frac{n}{2}\,.\,\frac{n+1}{3}\,\frac{y^3}{x^{n+2}}\ldots\ldots\ldots\ldots\ldots\ldots\right)$$

$$+ m\,a^{m-1}\,b\left(\frac{1}{x^{n-1}} - \overline{n-1}\,\frac{y}{x^n} + \overline{n-1}\,.\,\frac{n}{2}\,.\,\frac{y^2}{x^{n+1}}\ldots\ldots\right)$$

$$+ m\,\frac{m-1}{2}\,a^{m-2}\,b^2\left(\frac{1}{x^{n-1}} - \overline{n-1}\,.\,\frac{y}{x^n}\ldots\ldots\right)$$

$$+ m\,\frac{m-1}{2}\,.\,\frac{m-2}{3}\,a^{m-3}\,b^3\left(\frac{1}{x^{n-1}}\ldots\ldots\right)$$

$$+ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

And since $(x+y)^n = (a+b)^m$, if we expand each side and equate the first terms, there will result $x = a^{\frac{m}{n}}$ ....(2)

$$\text{and } y = \frac{m}{n}\frac{x\,b}{a} + \frac{m}{n}.\frac{m-1}{2}\frac{x\,b^2}{a^2} + \,..\; - \frac{n-1}{2}\frac{y^2}{x} - \,.. \quad \text{or} \quad \frac{m}{n}\frac{x\,b}{a} + \frac{m}{n}\frac{m-1}{2}\frac{x\,b^2}{a^2} + \ldots\ldots - \frac{m}{n}\,\frac{n-1}{2}\frac{x\,b^2}{a^2} - \,.. \quad (3)$$

Now since $x + y = (a + b)^{\frac{m}{n}}$, it is evident that the terms of equation 1, properly taken, ought to give the development of $(a + b)^{\frac{m}{n}}$; and this will be the case, if we collect the terms in the vertical order of their collocation, and make the necessary substitutions from equations 2 and 3.

The first term gives $\frac{a^m}{x^{n-1}} = a^{\frac{m}{n}}$; and the second term, without its coefficient, will be $a^{\frac{m}{n}-1}\,b$; and thus it may easily be seen that the powers with a fractional positive exponent follow the same law as those whose exponents are whole numbers. Therefore, for the sake of brevity, we shall omit the powers of *a* and *b*, and consider the coefficients only.

By substituting from equation 3, the coefficient of the second term becomes $-\frac{\overline{n-1}\,.\,m}{n} + m = \frac{m}{n}$.

In like manner the coefficient of the third term, by taking the coefficients of the three next vertical terms, is equal to $\overline{n-1}\,.\,\frac{n}{2}\,.\,\frac{m^2}{n^2} - m\,.\,\overline{n-1}\,.\,\frac{m}{n} + m\frac{m-1}{2} = \frac{m}{n}\,.\,\frac{m-n}{2}$. But here we have taken no notice of the coefficient of the second term $\frac{m}{n}.\frac{m-1}{2}\frac{x\,b^2}{a^2} - \frac{n-1}{2}.\frac{m^2}{n^2}\frac{x\,b^2}{a^2}$ of the value of *y*, which was neglected in the former substitution for the coefficient of the second term. If, therefore, we add this coefficient, multiplied by $-\overline{n-1}$ the coefficient of $\frac{y}{x^n}$ in equation 1, to $\frac{m}{n}\,.\,\frac{m-n}{2}$ found from summing the three vertical terms, we shall have $\frac{m}{n}\,.\,\frac{m-n}{2n}$ for the entire coefficient of the third term of the development of $(a+b)^{\frac{m}{n}}$; that is, the same as would be given by the general theorem for whole positive powers by changing *n* into $\frac{m}{n}$. And by following the same course we shall discover a like coincidence in the coefficients of the fourth and other terms. Whence the theorem is true generally for positi[illegible]ns; and that it is equally so for negative may be shown [illegible]; therefore, it is universally true for all whole [illegible] numbers.

Because all fractions, whether mixed, compound, or continued, may be reduced to simple fractions, having their numerators and denominators whole numbers, and the theorem has been proved to be universally true for such fractions; and because every irrational number may be either accurately or so nearly expressed by a fraction that the difference shall be less than any assignable quantity, it follows that the theorem is true for all numbers rational or irrational.

To extend this theorem to imaginary exponents, we must observe, that as the form of an irrational exponent is not changed by making it imaginary, so neither is the form of any coefficient which is a function of this exponent; consequently the theorem is likewise true for imaginary powers, and is, therefore, universally true.

A demonstration of the binomial theorem might easily have been given for fractional powers, by pursuing the same route that I have for the demonstration of whole numbers; namely, by extracting the successive roots, and observing the law which connects the quotients of the coefficients of the succeeding by those of the preceding terms. I have, however, chosen the present method, because it is more simple and natural, and because it exhibits a connective dependance between the proofs of whole and fractional numbers that was supposed not to exist, and displays the resources of the elementary branches of a science, which has itself, for this purpose in its full extent, often been thought to be not sufficiently general.

ARTICLE VI.

ANALYSES OF BOOKS.

Recherches sur l'Identité des Forces Chemiques et Electriques. Par M. H. C. Œrsted, Professeur a l'Université Royale de Copenhagne, et Membre de la Societé Royale des Sciences de la même Ville, &c. Traduit de l'Allemand par M. Marcel de Serres, Ex-Inspecteur des Arts et Manufactures, et Professeur de la Faculté des Sciences á l'Université Imperiale; de la Societé Philomatique de Paris, &c. Paris, 1813.

In the fifth volume of the *Annals of Philosophy*, p. 5 (Jan. 1815), I gave some account of this work, mentioning at the same time that I had not seen the book itself, but derived my information from the German journals, and from an outline given by Von Mons in his translation of Sir H. Davy's elementary work on chemistry. Some time after this notice of mine appeared, I received a letter from Professor Œrsted informing me that the account of his book in the German journals was far from accurate, and that I should probably have formed a more favourable opinion of it than he conceived I had done if I had perused the work itself. To put it in my power to do so, he promised to send me the French translation of it, which had been made under his own eye, and was, he said, in many respects, superior to the original. In consequence of this letter, to which I returned the proper answer, I received, some months ago, the work, of which I have transcribed the title page at the commencement of this article, and I now sit down to give the best analysis of it which I can, both for my own sake, and for the advantage of my readers. It gives me no small degree of pleasure to have it in my power to do justice to Professor Œrsted, whose knowledge of the science of chemistry, and whose powers of arrangement and generalization, are very uncommon. The book is highly worthy the perusal of all those British chemists who aim at the improvement and the perfection of their science. It is rather surprising that a work of such originality and value should have remained for these four years quite unknown in this country; for I am not aware that any notice of it has been taken either in Great Britain or in France, except the very imperfect and inaccurate outline which I gave in my Sketch of the Improvements of Chemistry for the Year 1815.

M. Œrsted considers the state of chemistry to be similar to that of mechanical philosophy before the appearance of Galileo, Descartes, Huygens, and Newton. Many important facts were known before the time of these illustrious philosophers; but these facts had not been reduced to their simplest principles; the connexion between them was not perceived; the fundamental laws were not discovered. At present, mechanical philosophy is brought to such a state of perfection that it embraces all the movements of the universe as a great mechanical problem, the solution of which enables us to calculate beforehand a vast number of particular phenomena.

Hitherto the object of chemists has been to bring all the effects under the action of affinities, as the last limit that can be attained; but scarcely any experiments have been made upon affinity in general: no connexion has been shown to exist between affinities: it has not been possible to reduce them to one general principle, from which all the phenomena can be deduced. The object of the work, of which I propose to give an account, is to commence this important generalization. He adopts that explanation of chemical phenomena known in Germany by the name of the *dynamic system*, which he considers as having been first started by Ritter, and as having been fully established by the great galvanic discoveries of Berzelius and Davy.

The work is divided into nine chapters, of each of which I shall give a successive analysis.

Chap. I.—*Of the Method of classifying Inorganic Bodies according to their Chemical Nature.*

As the object of chemical research is to discover the nature and properties of bodies, it is of great importance to present them in the most methodical order. Hitherto bodies have been classed according to certain characters, and attempts have been made to define the classes thus formed. By this manner of proceeding, a certain degree of order has been introduced into chemistry, but by no means suited to the present state of the science. In fact, these definitions, according to which it has been attempted to class every thing, were established at a time when the science had made but little progress, and when the object was merely to arrange bodies in certain isolated groupes. While matters continued in this state, it was easy to make the definition agree with the things, and the limits of the definitions might be in some measure regarded as limits assigned by nature herself. But when new discoveries filled up the gaps which existed between these groupes, these definitions required to be modified or extended; and after all they could not be made to apply correctly. Some were abandoned altogether, and others were retained, which, however, are not more capable than the rest of withstanding a rigid examination.

When Boerhaave and Stahl began their chemical career, only six perfect metals were known, and the remaining metallic bodies were excluded from the class on account of their brittleness. This distinction continued till the discovery of a variety of new metals, and filled up the great gap between the ductility of gold and the brittleness of arsenic. It was then presumable that the intervals still remaining would be gradually filled up by new substances, and consequently that the ductile and brittle metals could no longer constitute two separate classes. A great degree of volatility was also considered as sufficient to exclude various bodies from the class of metals. But at present when we know that even gold itself may be volatilized by electricity, by powerful burning glasses, and by the heat excited by means of oxygen gas, we cannot consider want of volatility as essential to the metallic nature of bodies. The temperature at which gold is volatilized is some hundred *thermic metres* above that at which arsenic or mercury is converted into vapour. By *thermic metre*, Prof. Œrsted understands the thermometric space comprehended between freezing and boiling water. This space he considers as unity, and thus uses a language that applies equally to all the different thermometrical scales which are employed in different countries.

If the mean temperature of the earth were five thermic metres higher than it is, arsenic and mercury would be always in the state of vapour; and yet the ratio between the vapour point of gold and the temperature of the atmosphere would not vary much. It is possible, therefore, that metals may be discovered, which exist only in the state of vapour. Volatility then, or fixedness, can never enter into the definition of metallic bodies.

Opacity, and the property of conducting electricity, which belong to the metals, exist in them in different degrees, as is obvious from the transparency of gold leaf and from the difference between the conducting power of copper and iron. Such properties, therefore, cannot enter into the definition of a metal.

What then, it will be asked, constitutes a substance a metal? Obviously its resemblance to other metallic bodies. It was by this successive comparison of bodies, as they were discovered, with those already known, that the class of metals was formed and extended. Unless this had been the case, the characters ascribed to them could not have experienced so many variations. Definitions are merely modes of making ourselves understood. They do not constitute limits really fixed by nature. As the science advances, therefore, we must overleap these artificial bounds placed in our way by our predecessors. We must continue our comparisons. The limit of yesterday ought not to continue our limit to-day, if the new progress of the science requires a new one. Neglecting, therefore, all artificial distinctions, we shall take as the basis of our classification some substance easily distinguishable, and we shall place next it some body that resembles it most; and we shall go on in this way as long as it shall be possible to proceed.

To be better understood, let us apply these principles to the class of metals. There is obviously a very great difference between the properties of gold and arsenic. The former is the most ductile of inorganic bodies, while the latter is so brittle as to be with great facility reducible to powder. Gold is exceedingly fixed, and cannot be volatilized by any heat which we can raise in our furnaces, while arsenic is volatilized by a heat of 2·82 thermic metres. Gold is so little combustible that this property could not have been recognized in it except by means of electricity, or some of the most *burning* acids; while arsenic is so inflammable that it burns with a strong flame at a temperature comparatively low. If we compare arsenic with phosphorus and sulphur, we shall find the difference between it and these bodies much less striking. Phosphorus and sulphur indeed have a certain degree of transparency, and are bad conductors of electricity; while arsenic is opaque, and an excellent conductor. But how many points of agreement do we find to make up for these differences? All the three are volatile, have a strong smell, and act with energy upon living bodies. When united to oxygen, they form acids; the acids which contain a maximum of oxygen are in all of them very fixed, while those containing less oxygen are volatile. Finally they combine readily with metallic bodies, which is not the case with those substances that contain oxygen as a constituent.

When we consider the metals with regard to their combustibility, we get a new example of a similar gradation of a property in the same series. From the most combustible metal to gold, the metals form a series of gradations, the terms of which are sufficiently known. When we come to gold, we may continue the series by platinum, which is still less combustible than gold. Then come osmium and iridium, in which the combustibility is so weak that acids are incapable of attacking them; though the alkalies favour their oxidation in a state of incandescence. Having thus come to a point in the series where the combustibility in some measure disappears, we may continue it to the most absolute incombustility. A body is perfectly incombustible when its presence is necessary for combustion going on.[*] Among the *unburnt* bodies which we know, oxygen is the only one that we can consider as incombustible.[†] If it should be discovered hereafter that oxygen may be *burned* by means of another principle, it will not be the less true that the body whose properties are most opposite to those of combustible bodies is the only really incombustible substance. Oxygen, therefore, must obviously terminate the series of undecomposed bodies as being perfectly incombustible, and thus at the greatest possible distance from the body with which the series commenced.

Thus Professor Œrsted makes the same division of simple bodies that I have done in my System of Chemistry; namely, into *combustibles* and *supporters;* which last bodies he calls bodies possessing *la proprieté comburente.* I think the term which I have been in the habit of using is better suited to our language than any translation of his appellation which I could have adopted. It is gratifying to observe such a subdivision advanced by a gentleman that in all likelihood was unacquainted with my arrangement; because it adds greatly to the probability that the arrangement itself is founded in the nature of things. Œrsted's series of simple substances then begins with the most perfect combustible, and terminates with the most perfect supporter.

Some bodies are very combustible in certain circumstances, while they are very little so in others. We must, therefore, establish a comparison between these bodies while they are in the same circumstances. Carbon is capable, for example, of reducing the greater number of the metals, and yet it cannot be considered as a very combustible body; for at the ordinary temperature of the air, it is less combustible than gold or platinum. It is well known that few acids have any action on carbon, and that this action is very feeble. We observe likewise, that those bodies in which carbon predominates, constitute a negative element in the galvanic chains even when opposed to gold or silver. This proves, according to a general law, that carbon is less oxidable than gold or silver. Sulphur burns readily at a high temperature; but at the ordinary temperature of the air, it is less combustible than any of the metals; and even at a high temperature its affinity for oxygen is much weaker than that of carbon. Sulphur, then, may be assimilated to less oxidable bodies; for the increase of its combustibility in high temperatures is singularly favoured by the diminution of its cohesion from heat, and by the tendency of sulphurous acid to assume the gaseous form. The gaseous nature of the acids formed, and the attraction of sulphur for various metals, must contribute materially to the deoxidation of different metallic oxides by carbon and sulphur. The feeble combustibility of carbon and sulphur is indicated likewise by the little contraction which these two bodies sustain when united to oxygen. Sulphur possesses also the remarkable property of disengaging heat and light when it combines with different metals; showing that it possesses a good deal of the *supporter* in its nature. It forms an acid likewise when it unites with hydrogen, which establishes a new analogy between it and oxygen. Tellurium exhibits a similar property, and of course must be placed near sulphur in the series.

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[*] Iodine was unknown when this book was published; and though Œrsted was aware of Davy's hypothesis respecting chlorine, he at that time leaned to the old doctrine, for reasons which he assigns; but which do not seem of much weight.—T.

[†] My readers will understand what Prof. Œrsted means when I mention that his *perfectly incombustible* bodies are those which I call *supporters of combustion.*—T.

I have entered into considerable details respecting the first of Professor Œrsted's series; that, namely, which consists of the undecompounded bodies. The character which distinguishes them is combustibility, or the property of supporting combustion. The combustible bodies unite with the supporters in general; they unite with each other; and their union takes place with considerable energy. They constitute a series of affinities apart which ought to be examined separately. It will not be necessary to enter into so minute an account of the other two series into which our author distributes the remainder of chemical bodies. It may be sufficient to observe, that he has adopted the two divisions of *primary compounds* and *secondary compounds,* which I have given in my System. The primary compounds constitute his second series, consisting of those bodies which he has called *corps brulées.* These primary compounds consist of two sets of bodies; namely, *acids* and *bases,* as I have particularly explained in the last edition of my System of Chemistry. For it is gratifying to find, that without being aware of what M. Œrsted had previously done, I have given exactly the same kind of division as he had done. I was obviously led to my arrangement in a great measure (or at least into the division of bodies into acids and bases) by Berthollet's observations in his treatise on affinity.

And Mr. Œrsted acknowledges his obligations to the same work. This may serve, in some measure, to account for the similarity of our opinions.

The primary compounds, or the *corps brulées*, as Œrsted calls them, may, he thinks, be also arranged in a series beginning with those bodies which possess the character of alkalinity in the greatest perfection, and terminating with those which are most completely acid. Of course the bodies in the middle of the series possess but little either of alkalinity or acidity, or in other words, combine with little energy either with acids or alkalies; but are notwithstanding capable of entering into combination with both. An acid, of course, is a body capable of combining with, and of neutralizing the properties of, alkalies; while an alkali or a base is a body capable of uniting with, and neutralizing the properties of, an acid. This was the definition given long ago of these bodies by Sir Isaac Newton. It is the notion adopted by Berthollet in his Chemical Statics. It is the opinion of Œrsted, and is the only opinion which the present state of our knowledge will admit. Of course bodies of this series are capable of uniting with each other, and those at the two extremities of the series unite with most energy. The affinities which they exert are of a peculiar kind. They have been more studied than any other department of chemistry. The compounds which they form are usually called *salts*.

The third series of M. Œrsted consists of those bodies which I have distinguished by the name of *secondary compounds*, and to which he is satisfied with giving the name of *salts*.

Thus Œrsted divides chemical bodies into simple primary compounds and secondary compounds; and he thinks that the bodies belonging to each may be disposed in a regular series.

In the first series there are many ductile bodies; in the second and third series there are none.

Most bodies in the first series are opaque; most of those in the two others are transparent.

Those belonging to the second class are (with a few exceptions) much less fusible than those of the first class, and at the same time much harder. Those of the third class are much less fusible than we should expect from the fusibility of their constituents, especially when composed of the most energetic acids and alkalies.

The bodies of the first class are usually good conductors of electricity. Those of the second class are almost all bad conductors while they remain solid, but become better when reduced to a state of liquidity; though not so good as those of the first class. Those of the third class are all bad conductors while solid; but when they contain much water, they acquire the property of conducting electricity.

CHAP. II.—*Of the Chemical Forces.*

The most astonishing of all the forces which produce the chemical effects is *fire*. The kind of combustion which has been hitherto almost exclusively studied by chemists, is that which results from the union of a burning body with oxygen. To express the cause of this phenomenon, we say that the combustible body has an affinity for oxygen; and that oxygen has an affinity for the burning body. After a body has burned for a certain time, it loses its faculty of burning any more in the same circumstances. This change is expressed by saying, that the body is *saturated with oxygen*. This phrase means merely that the attraction of the combustible substance for oxygen has become so weak that it is no longer capable of overcoming the forces opposed to it. But in more favourable circumstances, the combustion of the same body may proceed further. Even in this case, it would find a limit; and the same thing would take place in every supposed situation, till at last the property of burning in the body would be completely destroyed. From this we learn, that the attraction of the burning body for oxygen is weakened or even annihilated by an activity which exists in the oxygen. In the same way the attraction of the oxygen for the burning body is destroyed by an activity residing in the burning body. Thus these two forces (that in the oxygen and that in the burning body) have the property of mutually neutralizing each other. In many cases the neutralization is so complete that we can neither detect in the compound the property of burning, nor that of supporting combustion. Now in *physics*, those forces which mutually destroy each other are called *opposite forces*. The same mode of speaking ought to be introduced into chemistry: the attraction of combustible bodies for supporters is not the only common property which they possess; there are several others which disappear and reappear along with this attraction. Thus the property of acquiring electricity by contact with conductors, that of uniting with other combustibles, and that of acting strongly upon light, may be mentioned as instances. If we were to explain these phenomena by saying that they depend upon the attraction of the burning body for oxygen, we should not express every thing which results from the nature of a combustible body. We shall, therefore, call this property *combustibility*, and the activity which distinguishes it the *force of combustibility*. For the same reason the attraction of oxygen for combustible bodies, and all similar attractions, may be called the *burning force* (*force comburente*).

Combustion then is produced by the mutual attraction which exists between the *burning force* and the *force of combustibility*, forces which have the property of destroying each other, and which for that reason ought to be called *opposite forces*.

The combination of bodies with oxygen is not only accompanied by a suppression of forces, but the compounds pass into another class, and exercise another series of affinities. Some of these compounds become alkalies; while others become acids. Now the alkalies and acids are capable of neutralizing each other; and, therefore, possess opposite forces. It may, at first sight, appear unaccountable that the same operation should produce two kinds of forces quite opposite to each other. M. Œrsted is led to what he considers as the true explanation by the following facts:

1. All those bodies that become strong alkalies by combustion have the property of decomposing water and depriving it of its oxygen. Such bodies must of course possess a great degree of combustibility. But all the bodies that become acid by combustion have little action on water, unless favoured by peculiar circumstances. They are, however, oxidized in the air with the greatest facility, and this oxidizement is singularly promoted by heat.

2. Those bodies that become alkaline unite with only a small quantity of oxygen, while those that become acid unite with a great quantity of that substance.

3. Those oxides which possess alkalinity in the greatest perfection are not saturated with oxygen. Those saturated oxides that combine with acids are capable of being separated from acids by much weaker forces than the non-saturated oxides. In the oxides of bodies moderately combustible, and which are not combined with much oxygen, we see acidity and alkalinity existing at once. Very combustible bodies saturated with oxygen form compounds (water for example) neither acid nor alkaline.

From the consideration of these facts, M. Œrsted concludes, that those products of combustion which still possess an *excess* of the *force of combustibility* are *alkaline;* while those in which that force is perfectly destroyed, and in which the *burning force,* on the contrary, is in *excess,* are *acid.* In a certain state of equilibrium of these forces there is an equilibrium of acidity and alkalinity. But our author is of opinion that we must not merely attend to this state of the forces, but take into consideration that the forces by the effect of combustion are brought into a state of activity quite new; for the force of combustibility no longer acts as such in the alkalies, nor the burning force in the acids. Sometimes indeed we see both kinds of forces in the same substance. Thus in ammonia we find both combustibility and alkalinity existing together, and in nitric acid we have an example of the burning force and acidity in the same substance. In some saturated oxides where the burning force of the oxygen is but little restrained by the contrary attraction, we see it exhibiting almost all its force, and yet the oxide exhibits no signs of acidity. We have an example of this in the peroxides of lead and manganese. One of the forces ought then to be limited, and (to speak so) reduced to an inferior power by the action of the other before it can produce either alkalinity or acidity.

I shall not enter into our author's observations on the intensity and capacity of acids and alkalies, because this part of the subject has been much simplified by the improvements introduced into the atomic theory since the work under review was published.

(*To be continued.*)

Article VII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

April 1.—A paper, by Dr. Brinkley, was read, entitled "Results of Observations made at Trinity College, Dublin, for determining the Obliquity of the Ecliptic, and the Maximum of the Aberration of Light." After some general observations upon the obliquity of the Ecliptic, the author proceeded to consider the opinion of astronomers that observations of the winter solstice have given a less obliquity than those of the summer—an opinion sanctioned by the observations of Maskelyne, Arago, and Pond; but questioned by Bradley. Dr. Brinkley referred this difference to some unknown modification of refraction, and stated that he has observed that at the winter solstice the irregularity of refraction from the sun is greater than from the stars at the same zenith distance; whence he inferred the necessity of paying greater attention to the observations made at the winter solstice. The author next alluded to the maximum of the aberration of light, which he stated, from observations made by him during the last year, to be 20·80″.

At this meeting also, a paper, by Sir E. Home, was read, entitled "Some additional Remarks on the Skeleton of the Proteorrhachius." The author commenced by stating, that having previously proved that this animal has four legs, and that its progressive motion through water is similar to that of fishes, he was led to look for its place in the scale of beings between amphibia and fishes. He found the vertebræ of the proteus cupped at both extremities, like those of the fossil animal; and from this and other circumstances it appeared, that the fossil animal was nearly allied to the proteus; but the capacity of the chest, and the want of sufficient room between the occiput and first rib, seemed to show that it breathed by lungs only and did not possess gills. From this circumstance, and from its appearing to have been capable of the two kinds of progressive motion, the author gave it the name of proteorrhachius.

Another paper was likewise read at this meeting, on some new methods of investigating the sums of several classes of infinite series, by C. Babbage, Esq. From the nature of the subject, this paper did not admit of being read in detail. But the object of the author appears to have been to explain two methods of finding the sums of a variety of infinite series. One of these was discovered several years ago; but finding that some of the results to which it led were erroneous, he did not then publish it. On inquiring into the cause of these errors, he detected the second method. The cause of the fallacy was afterwards discovered, and a rule was proposed for judging of the truth of the results, and a mode of correcting them when found to be erroneous. The author stated that nearly similar results were found by MM. Poisson and Lagrange, but that neither of these mathematicians had explained the cause of the error, or given a method of correcting them.

The Society adjourned till after Easter.

GEOLOGICAL SOCIETY.

Jan. 15.—A paper was read, from S. Babington, Esq. "On the Geology of the Country between Tellichery and Madras."

The face of the country in general below the ghauts is marked by low rounded hills, composed of a porous substance called, by Buchanan, laterite. The mountains denominated ghauts, and the other mountains traversed in the course of his journey, the author describes as consisting of granite, gneiss, mica slate, &c. varieties of horneblende rock sometimes containing garnet, and in one place cyanite. The Carnatic, or country east of the eastern ghauts, is flat, as though it had been once covered by the sea; and in digging a well about two miles from the coast, a stratum of brown clay was first cut through to the depth of about five feet, then a stratum of bluish black clay nearly 30 feet, containing beds of oyster, cockle, and other shells; and at about 37 feet from the surface water is obtained.

Feb. 19.—A paper was read from the Hon. W. I. H. F. Strangways, on the Rapids of Imatra on the Voxa river, in Carelia, N.W. of St. Petersburgh, with an outline of the probable history of their formation, and a notice of the bursting of the lake Loubando into the Ladoga in 1818.

The greater part of the course of the Voxa may be considered rather as a chain of lakes than a river; near Imatra it is contracted into a narrow channel within rocky banks, about 60 feet in breadth, which continues about 500 yards; the eastern bank is a section of a table land of inconsiderable extent, deeply channelled and covered with pebbles and bolders of great size, some of which are hollowed into the most fanciful shapes. The river rushes with great fury and a tremendous noise through this channel; the rock through which it passes is the common red granite of Finland, which is easily disintegrated by mere exposure to the weather, and hence may have presented no obstacle to the current of water from the higher land. In 1818 one of the lakes, Loubando, which discharged its waters eastward into the Voxa, opened a passage into Lake Ladoga eastward, by bursting through the isthmus of Taipala, a circumstance that will probably alter its future geographical character.

A paper was also read, from Dr. Adam, of Calcutta, "On the Geology of the Banks of the Ganges from Calcutta to Caunpore."

There is no rock on the banks of the Hoogly or Ganges between Calcutta and the province of Bahar. The soil consists of a mixture of argillaceous earth, sand, and minute grains of mica, and is highly favourable to vegetation.

After leaving the low lands of Bengal, the Ragemaal chain of hills present themselves; of these, as well as other hills between this chain and Monghyr, the author has sent a series of specimens as a necessary illustration of his paper.

After leaving Monghyr, the country again becomes flat, and continues so for upwards of 200 miles; at Chenor, there are several low ranges of hills; between these and Caunpore, there is neither rock nor rolled stone; but the soil consists chiefly of clay, sometimes considerably indurated.

March 5.—An extract was read of a letter from Whitby, from the Rev. George Young, addressed to Samuel Parkes, Esq. containing an account of the discovery near Whitby of the fossil remains of an animal, supposed to have been an ichthyosaurus.

The fossil is described as imbedded in the alum rock, the skull being entire, and measuring two feet ten inches long, one foot in breadth at the broadest part, and tapering to a point like a bird's beak; the jaw bones have been twisted, the teeth broken and displaced, and the remainder of the skeleton is much mutilated and imperfect. It is supposed that the animal must have been at least 14 feet long.

A paper was read, from H. T. De La Beche, Esq. on the rocks with their fossils of the coast extending from Bridport Harbour, Dorset, to the eastern point of Torbay, Devon.

The line of coast described, beginning at its western point, consists of the following beds which dip eastward.

1. Rock marl, or red conglomerate; this, at Axmouth Point, passes gradually into the lyas, which dips below the surface a little to the westward of Bridport Harbour; on this rests,

2. Green sand, which is found first covering only the tops of the hills; but on proceeding eastward, forms a continuous bed, and is surmounted at Axmouth Point by the

3. Chalk, into which the green sand sometimes passes; but the author has never observed the green sand passing into any inferior bed; on all the hills capped with green sand are found quantities of fragments of flint and chert, in some instances agglutinated together by a silicious cement forming a breccia.

Of the fossils, the author gives a partly descriptive catalogue, with *some drawings.*

The lyas produces the ichthyosaurus.

Some parts of another animal not described; a fish with rectangular scales, and one in which the scales have that form only towards the head.

The nautili and ammonites are numerous.

Pentacrini are also found.

Trochi occur rarely.

Casts of turbinated shells more common.

Pectens, gryphites, anomiæ, and other bivalves, in great abundance.

The fossils of the green sand are numerous.

Article VIII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *New Acid of Sulphur.*

Gay-Lussac and Welther have discovered a new acid combination of sulphur and oxygen intermediate between sulphurous and sulphuric acid, to which they have given the name of sulphurin acid. If we consider sulphurous acid as a compound of four volumes sulphur and four volumes oxygen gas, sulphuric acid will be a compound of four volumes sulphur and six volumes oxygen. Hence it is probable that this intermediate acid will be a compound of four volumes sulphur + five volumes oxygen.

The sulphurin acid is obtained by passing a current of sulphurous acid gas over the black oxide of manganese. A combination takes place; the excess of the oxide of manganese is separated by dissolving the sulphurinate of manganese in water. Caustic barytes precipitates the manganese, and forms with the sulphurin acid a very soluble salt, which crystallizes regularly, like the nitrate or muriate of barytes. Sulphurinate of barytes being thus obtained, sulphuric acid is cautiously added to the solution, which throws down the barytes, and leaves the sulphurin acid in the water. This acid may be concentrated very considerably without any loss.

II. *New Compound of Oxygen and Hydrogen.*

Thenard in the course of his experiments on the oxygenized acids,* &c. is stated to have placed beyond a doubt the existence of a new compound of oxygen and hydrogen, consisting of two atoms of oxygen and one of hydrogen. It is a fluid less volatile than water, and soluble in it in any proportion: hence it may be obtained nearly free from that liquid by placing the mixture under the receiver of an air-pump with sulphuric acid. When separated from water and concentrated as much as possible, its sp. gr. is 1·417. It destroys or whitens all organic substances. When a drop of it is allowed to fall upon oxide of silver, the oxide is decomposed, with explosion, and often with emission of light.

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* See the present volume of the Annals, p. 1.

III. *Wavellite.*

From Sir H. Davy's analysis of this mineral, it has been considered as a hydrate of alumina. The want of the usual ratio between the water and the alumina led Berzelius to suspect the presence of an acid in it. He accordingly subjected it to an analysis which he found attended with much greater difficulty than he had anticipitated. He succeeded, however, at last, and found it a subphosphate of alumina mixed with a little neutral fluate of alumina. The following is the result of an analysis:

Alumina	35·35
Phosphoric acid	33·40
Fluoric acid	2·06
Lime	0·50
Oxides of iron and manganese	1·25
Water	26·90
	99·46

IV. *Plombgomme.*

A mineral, known by this name, which is found at Huelgöet, has been hitherto considered as a compact wavellite. Berzelius subjected it to analysis, in order to ascertain whether it was also a phosphate of alumina. He found its constituents as follows:

Alumina	37·00
Oxide of lead	40·14
Water	19·90
Sulphuric acid	0·20
Oxides of manganese and iron	1·80
Silica	0·60
	99·64

It is, therefore, an aluminate of lead with water of combination, just as spinelle and gahnite are aluminates of magnesia and of zinc, but without water. Sulphuric acid appears to have penetrated it in small quantity during its formation, and appears to be equally united with the alumina and the oxide of lead. This is the first example of this acid occurring in a mineral not volcanic.

V. *Euclase.*

The constituents of this rare mineral, according to a recent analysis of Berzelius, are as follows:

Silica	43·32
Alumina	30·56
Glucina	21·78
Oxide of iron	2·22
Oxide of tin	0·70
	98·58

Hence it is a compound of one atom of silicate of glucina and two atoms of silicate of alumina.

VI. *Crichtonite and Elba Iron Ore.*

The mineral called *crichtonite* by Count Bournon has been ascertained by Berzelius to be a titaneous iron. As this mineral has a peculiar metallic brilliancy similar to that of iron ore from Elba, it occurred to Berzelius that the Elba iron ore probably contained titanium also. An analysis of it soon satisfied him that this opinion was well founded.

VII. *Potter's Clay near the Halkin Hills, Flintshire.*

In the number of the *Annals* for March, p. 233, the discovery of this clay was noticed, and likewise the circumstance of its being adapted for the manufacture of stone ware without any addition. I have received a very small specimen of the clay, sufficient, however, for a chemical analysis; and I shall here state the constituents which I found it to contain. Its characters being the same with those of potter's clay, it seems needless to give any description of its appearance. Indeed the minute specimen which I have in my possession would not enable me to give its characters with much precision. Its constituents were as follows:

Silica	14·400	57·60
Alumina	6·100	24·40
Oxide of iron	0·500	2·00
Lime	0·480	1·92
Moisture	2·375	9·50
	23·855	95·42

It appears from this analysis that the clay in question contains a considerably smaller proportion of alumina than potter's earth usually does. Notwithstanding the two per cent. of oxide of iron which it contains, it may be exposed to a strong red heat without losing its white colour. This property of retaining its colour, when heated, is essential to every clay which is to be used for making stoneware.

VIII. *Persulphates of Iron.*

In consequence of Mr. Cooper's paper on the persulphates of iron in the last number of the *Annals* (p. 298), which contains the important discovery of a crystallized persulphate of iron, I think it necessary to state, that in the month of October last year, Mr. Rennie, a friend of mine, a surgeon in Glasgow, a very ingenious man and fond of chemistry, brought me a few crystals, which were obtained, he said, by evaporating green vitriol repeatedly in the open air, redissolving it by means of sulphuric acid, concentrating the solution, and setting it aside. He succeeded only once in obtaining these crystals, and he considered them as crystals of persulphate of iron. These crystals were regular octahedrons (as far as could be determined by the eye), they were transparent and colourless, and had very much the taste and appearance of alum crystals. The whole quantity which I got did not exceed a grain in weight. I dissolved one of these crystals in distilled water, added to the solution an excess of caustic potash, heated and then poured the colourless liquid off the peroxide of iron which had been precipitated. On adding sal ammoniac to this liquid, I got a white precipitate, which appeared to the eye little less abundant than the preceding precipitate of peroxide of iron. From these experiments, which were all that the minute quantity of crystals in my possession admitted of, I considered the presence of alumina in them as ascertained. Hence I was led to suspect the presence of alum, to conclude that the salt was a mixture of alum and persulphate of iron, and that the crystalline form was owing to the alum. This conjecture of mine, which from Mr. Cooper's experiments we see was inaccurate, prevented Mr. Rennie from making his discovery known at the time. I have still one or two of the crystals which he gave me in my possession.*

I may take this opportunity of stating, that the persulphate of iron, which I described in *Annals of Philosophy*, xii. 462, was the same as Mr. Cooper's in composition, though the shape of the crystals was different.—T.

IX. *Gauze Veils suggested as Preventives of Contagion.*

By Mr. Bartlett.

(To Dr. Thomson.)

SIR,

Permit me, through the pages of your *Annals*, to suggest to your medical readers, and those employed in Hospitals and other infected places, the practicability of using *gauze veils* as a preventive of contagion. This method was successfully employed by M. de Saussure and his party when he ascended Mont Blanc, to preserve their faces from excoriation; nor was their sight at all impaired, as is usually the case with travellers in elevated regions. When, therefore, we perceive the efficacy of a contrivance so simple in the rarest of mediums, and descend to the greatest depths of the earth to which the labour or ingenuity of man has penetrated, and find the same means made use of (viz. *gauze* wire) to prevent the combustion of the inflammable gases which abound there; why, let me ask, may it not be employed to the end proposed? It is an ascertained fact that the miasmata which stagnant waters exhale is diverted by the intervention of a few shrubs only.* It is also well known that travellers are preserved from the suffocating heat of the sinoe of the desert by merely bringing their faces in contact with the surrounding sand, the minute particles of which, in all probability, prevent the vapour from penetrating to the respiratory organs. Thus we find that all media exhibit the same phenomena when opposed by the same difficulties; and, as far as reasoning from analogy will admit an inference, I cannot help subscribing to the belief of the practicability of what I propose. I beg leave, however, to submit it with great deference to the readers of the *Annals of Philosophy*, sincerely hoping that should they deem it worthy of experiment, the result will be successful, since it would tend so materially to the advancement of the happiness of mankind.

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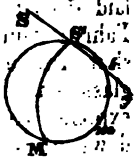
* I noticed these crystals before in *Annals of Philosophy*, xii. 461.

I have the honour to be, Sir,
Your very obedient servant,
J. M. Bartlett.

X. *On the Lunar Atmosphere.* By Mr. Emmett.

Hull, Feb. 15, 1819.



On Dec. 5, 1818, about 11 o'clock, the moon eclipsed a small star in the constellation Pisces, when the following appearances were observed. S′ *m* M being the illuminated part of the moon's disc, S′ the northern cusp, S S′ *y* the apparent path of the star; the contact took place at S′, and since the moon's latitude was about 2° 41′ S descending, the star was obscured for a very short space of time, the apparent path of the star cutting off a very small portion from the moon's disc.† the star did not disappear instantly, as is always the case when the stars' path approaches nearer the moon's centre, but continued in contact with the moon's limb for 25″ of time; for five or six seconds it gradually lost some of its brilliancy; then the form of a regular disc; then appeared like a minute ray of bluish light, slowly moving along the moon's limb, losing more of its brilliancy every moment, till at the end of about 25″, it disappeared in the most gradual manner. The star appears to have been kept in view, when really behind the moon's disc, by the refraction of her atmosphere. The emersion was not observed, the star having come into view about half a minute earlier than was expected.

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* Of this the Pontine marshes near Rome afford indubitable evidence, since whole families have resided near the spot for years without having suffered from the mephitic vapours which those putrid waters engender, and for which no other cause can be assigned than that a screen of *trees* separates their abodes from those pestilential wastes.

† A bare inspection of the figure is sufficient to show that the minute effect of refraction through an atmosphere of very little density, can only be observed when the versed sine of half the arc S′*t* is very small; when S′*t* does not amount to more than 3° or 4°, the star must appear upon the limb for a considerable time, if the moon have an atmosphere capable of refracting light.

The observation was made with a Newtonian reflector, of six inches aperture, and a very distinct power of 100: with this power, the star, before contact, presented a minute, round, well-defined disc, whose contact with, and change of place upon, the moon's limb, were most distinctly observed.

J. B. Emmett.

⁂ The existence of a lunar atmosphere is doubtful, and has been denied altogether by some astronomers. If it does exist, its tenuity must be extreme, as the brilliancy of stars for the most part is not in the least diminished by it. See in particular Col. Beaufoy's observations on this subject, *Annals of Philosophy*, ii. 225, *et passim*.

XI. *Mr. Murray on Dew, and on the Temperature of the Sea.*

(To Dr. Thomson.)

SIR, *Paris*, *Feb.* 15, 1819.

I was convinced of the truth of the late Dr. Wells's theory of the formation of dew from the first perusal of his very ingenious essay. Time has confirmed, not weakened the impression.

On the 5th of last month in crossing the Bochetta from Genoa to Turin, at half-past seven o'clock, a. m. with a still atmosphere and serene sky, I noted the following observation, which cannot I think be explained in any other way than upon the principles laid down by Dr. Wells. The external atmosphere was 27° Fahr.; that within the coach 54°. The windows had been shut for a considerable time. The exterior surface of the glass was dry, the *inner* covered with a thin *crust of ice,* though exposed to this medium of 54°. I lowered one of the side windows about *half* an inch; this had the effect of causing the ice to *disappear* very shortly. I explain the phenomenon in the following manner: The exterior surface of the glass radiated caloric to the heavens more promptly than it received the warm impressions from within, in consequence of which the respirable vapour condensed upon the inner surface passed into the state of ice. On admitting the external air, a current was established, and the ice dissolved, though it lowered the temperature considerably. The ball of the thermometer *in contact with the ice* within still supported a temperature of 54°. I should add to these that no ice formed on the surface of the *front* windows, and these were overshadowed by the covert of the cabriolet. Now Dr. Wells has clearly proved that a cloudy sky, or the prevalence of winds, are circumstances unfavourable to the formation of dew; and that an agitated atmosphere not only prevents the deposition of dew and the formation of hoar frost, ice, &c. but dissolves them as soon as formed.

Dr. Davy's ingenious researches on the temperature of the sea will no doubt be appreciated by the navigator. By this account we are apprized of the approximation of shoals by a decrement of temperature. This may be the case in the ocean, but circumstances concur, I am persuaded, to modify this law as applied to the approach to land. I kept an exact register of the temperature of the sea on my passage from the Mull of Galloway to Liverpool, and on my voyage from Leghorn to Civita Vecchia; and think I have clearly proved *that there is an increase of temperature in the sea off the mouths of rivers.* The mean of 14 observations made in St. George's Channel is 52·8°. On approaching N.W. buoy, the temperature was 55°, and successively rose to 60° Fahr. as we approached the river Mersey: here we were among *sand-banks.* Again: the temperature of the Mediterranean continued nearly uniform at 70·3° Fahr.; but off the river Ombrone, in Italy (even 10 miles at sea), the temperature rose to 71·5°. The experiments were made with care, and frequently repeated. I have the honour to be,

Your very humble servant, J. MURRAY.

XII. *On Galvanic Shocks.* By Mr. John Woolrich.

(To Dr. Thomson.)

SIR, *Lichfield, Feb.* 1, 1819.

In the last (fifth) edition of your System of Chemistry, I observe the following statement, vol. i. p. 174. Speaking of the properties of the galvanic battery, and the power of the plates in giving shocks, after remarking that the shock from several hundred pairs of plates is so violent as to be painful, you say, "Even in that case, if three or four persons take hold of each other's hands and form a chain, and if the two persons at the extremities touch each an end of the pile, they *alone* feel the shock, while the *intermediate persons are sensible of nothing.*" I have frequently formed a chain of eight or ten people, all of whom have felt the shock. As I conceive this erroneous statement has crept into your work through the hurry of compilation, nothing further need be said respecting it. I would take the liberty, however, of suggesting to you the propriety of noticing the error in the *errata* of the unsold copies; and, perhaps, a short notice of it also in your *Annals of Philosophy* would be adviseable for the benefit of those who have already purchased your last edition of Chemistry. I am, Sir, with great respect,

Your most obedient servant,

JOHN WOOLRICH.

XIII. *Notices communicated by C. Johnson, Esq.*

(To Dr. Thomson.)

SIR, *Lancaster, Feb.* 27, 1819.

1. *Meteorological Journal of Lancaster for* 1818.—I transmit you Mr. Heaton's table of the results of his meteorological journal for 1818. It contains no account of the quantity of rain, because one of two observers who used to furnish this information has removed, and the other has had the misfortune to injure his rain guage.

Tabular View of a Meteorological Journal kept at Lancaster. By Mr. Heaton.

1818.	THERMOMETER.						BAROMETER.					WEATHER.		WINDS.							
Months.	Highest.		Lowest.		Mean.	Mean diurnal variations.	Highest.		Lowest.		Mean.	Wet days.	Fair days.	South.	South-west.	West.	North-west.	North.	North-east.	East.	South-east.
	Day.		Day.				Day.		Day.												
Jan.	13	51°	1	27°	39°	7°	19	30·35	30	28·86	29·66	21	10	6	13	8	1	0	1	2	0
Feb.	18	51	4	21	38	9	11	30·18	2	28·89	29·61	14	14	4	12	5	0	0	2	0	5
March ..	29	55	10	32	41	10	31	30·45	4	28·23	29·45	20	11	5	11	8	3	3	0	1	0
April. ..	29	74	12	31	47	16	3	30·53	9	29·10	29·70	12	18	5	3	4	1	0	5	10	2
May	25	86	2	44	60	22	27	30·42	7	29 30	29·88	10	21	5	2	3	1	0	6	9	5
June....	10	80	24	50	63	15	6	30·40	19	29·41	30·00	12	18	6	8	12	0	0	1	3	0
July	17	88	3	51	65	15	15	30·33	26	29·75	30·03	17	14	6	8	16	1	0	0	0	0
August..	5	86	13	47	62	17	12	30·26	28	29·62	30·07	9	22	3	2	10	0	4	10	2	0
Sept.....	1	69	14	40	56	12	13	30·03	16	29·06	29·73	15	15	6	10	4	3	0	1	3	3
Oct.	2	65	7	39	55	10	29	30·33	5	29·25	29·85	15	16	11	7	2	0	0	0	5	6
Nov.	3	58	22	37	50	6	27	30·27	14	29·31	29·80	14	16	14	3	0	1	0	1	6	5
Dec.	1	57	29	27	40	5	29	30·66	6	29·29	30·05	12	19	6	4	1	3	0	8	0	9
	July	88	Feb.	21	51½		Dec.	30·66	Mar.	28·23	29·82	181	184	77	83	73	14	7	35	41	35

2. *Test of Gallic Acid and Silver.*—Gallic acid decomposes ammonio-nitrate of silver, or a solution of oxide, or of muriate of silver in ammonia, forming a very copious dense cloud and precipitate which does not entirely subside after a long time. A very minute quantity of silver, or of gallic acid, may be detected and distinguished in this way.

3. *Preparation of Polychroite.*—Polychroite* cannot be prepared by redissolving the *dry* extract of saffron in alcohol of sp. gr. ·800. If the extract be dried until it becomes brittle, a much weaker spirit must be used to redissolve it. When the alcohol is diluted to sp. gr. ·840, the extract deliquiates, but remains undissolved at the bottom of the flask, exhibiting a very beautiful purple colour. Probably sulphuric acid only produces this colour in a solution of the extract by abstracting the water.

4. *Action of Sherry Wine on Iron.*—A very respectable wine merchant of this town had a cask of sherry wine returned upon his hands in consequence of a gradual deterioration in taste and colour. On emptying the cask, the iron part of a cooper's tool (called a bracebit) was found at the bottom corroded in a remarkable manner at the steel extremity or head, but hardly, if at all, acted upon at the shank, which consists of malleable iron.

Mr. Phillips, at p. 113 of his valuable "Experimental Examination of the London Pharmacopœia," has stated, that "the solubility of iron depends very much upon its softness." But that very accurate experimenter and reasoner seems to have overlooked some solvents of iron besides tartar existing in sherry wine. Neither tartar nor vinegar has produced any effect upon a similar bracebit at all corresponding with that which took place in the sherry. I have had no opportunity of trying the gallic or malic acid, and think it better to state this trifling and probably useless fact than to offer speculation upon it.

I remain, Sir, your obedient servant,
CHRISTOPHER JOHNSON.

P.S. Can you favour your distant readers with a more detailed character or even title of Dr. John's Laboratorium than is to be derived from the article "Decomposition Chemical" in Napier's Supplement to the Encyclopædia Britannica?

* * *

A friend of mine who has just left Glasgow for Leipsic has promised to bring me a copy of this and several other German chemical books, which I have hitherto been unable to procure. When I receive it, I shall take an opportunity of giving an account of this work in a future number of the *Annals*.—T.

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* Thomson's Chemistry, iv. 50.

XIV. A correct Statement of the Temperature of the Interior of the Island of Cape Breton, taken at nine a.m. in the external Air, in a S.S.E. Inclination, from Dec. 5, 1813, to April 24, 1814.

1813.			1814.											
Dec.	5	48°	Jan.	1	22°	Feb.	1	8°	March	1	38°	April	1	47°
	6	50		2	46		2	41		2	25		*m* 2	49
	7	60		*c* 3	26		*e* 3	45		3	24		3	43
	8	50		4	16		4	13		4	20		4	45
	9	42		5	15		5	13		5	21		5	35
	10	44		6	11		6	27		6	30		6	41
	11	42		7	18		7	20		7	30		7	39
	12	43		8	28		8	24		8	42		8	34
	13	58		9	17		*f* 9	16		9	26		9	28
	14	56		10	18		*g* 10	30		10	38		10	55
	15	50		11	32		*h* 11	30		11	25		11	51
	16	46		12	50		*i* 12	24		12	34		12	41
	17	48		13	30		13	25		13	36		13	38
a	18	50		14	29		14	16		14	20		14	35
	19	54		*d* 15	31		15	25		15	19		15	40
	20	21		16	31		16	30		*k* 16	34		*n* 16	54
	21	22		17	32		17	31		17	20		17	51
	22	9½		18	40		18	30		18	19		18	47
	23	30		19	39		19	32		19	28		19	34
	24	38		20	37		20	40		20	34		20	37
	25	4		21	31		21	43		21	18		21	28
	26	6		22	28		22	23		22	13		22	39
	27	10		23	14		23	30		23	30		23	40
	28	12		24	24		24	26		24	28		24	38
	29	40		25	40		25	40		25	22			
	b 30	50		26	34		26	25		26	24			
	31	24		27	23		27	23		27	37			
				28	31		28	20		28	33			
				29	36		29	26		29	20			
				30	16					30	40			
				31	23					*l* 31	48			

a 25 inches snow on the ground.
b Much rain.
c Much snow and wind.
d This day I caught 51 eels.
e Grievous thaws; cellars full of water.
f At nine, p. m. the therm. stood 2° below zero.
g At five, a. m. 47°.
h Grievous thaw; much snow.
i Much wind.
k Very heavy rain.
l Fair.
m Sowed some seeds.
n Heavy rain.

XV. *New Mode of administering Medical Electricity.*

By Mr. Gill.

(To Dr. Thomson.)

No. 11, *Covent Garden Chambers,*
Feb. 20, 1819.

SIR,

The following extract of a letter from a gentleman at New York to a friend of mine here, and who has kindly permitted me to make what use of it I think proper, contains information of so much importance to humanity, and the medical and chemical world in particular, that I lose no time in communicating it to the public through your journal, as follows. It is dated Jan. 9, 1819.

"I was extremely sorry to find from your last letter that you had not recovered of your rheumatism. Have you tried electricity? It is found here to be a specific in all recent cases. It is, however, applied in the new manner I have once before spoken to you of; and whether I described the jar used or not, I do not now remember: for your advantage, I will now describe the mode of making it. The outside only of the bottle is coated with paper, having tin or copper extremely thin on its surface; the bottom of the bottle nor its inside has no coating. To the end of the wire which passes through the cork or cover, attach about half a yard of small brass wire so coiled up, that when it is thrust into the bottle it will expand itself against the sides. A bottle thus prepared will give shocks similarly to the tin foiled jars in this respect, that the strength is proportionable to the distance of the electrometer from the conductor; but its sensible effect is very different, as it affects the *muscles* of the limbs more sensibly than the *joints*, and it has been found to remove complaints which the common shocks would not reach."

My friend having also been kind enough to favour me with the former letter alluded to, which is dated Oct. 21, 1817; and finding that although the manner of constructing the jar is only hinted at, yet that the effects of it are more fully described; I have thought it proper to quote them as follows:

"An important improvement in medical electricity is said to be invented. It consists in pouring through the body, or diseased part, a large quantity of the electric fluid with scarcely any pain to the patient. This is effected by coating the jar with tinselled paper instead of tin foil, and by using imperfect or very weak conductors in making the circuit. The inventor has a patent for his invention, and I paid him five dollars for the secret. I have made some experiments on his plan, and I find the shock much modified, and sensibly different in its effect on the muscles."

I also learn from another letter to my friend, but from another correspondent in New York, and dated Jan. 9, 1819, that a "Mr. Everit has formed an establishment there for administering electricity in this *new and superior manner;* and the effect of which is beyond every thing that can be conceived. He has four machines in use."

I sincerely hope that this communication will induce such of your readers as may be possessed of the necessary apparatus, to fit up some jars in this novel manner; and that they will inform the public through your *Annals* of the results thereof; as it appears to me that the action of the electric fluid in this modification of it very much resembles that of the voltaic pile or battery; and may very possibly afford some valuable applications of it to chemical purposes. I am, Sir,

Your most obedient servant,
THOMAS GILL.

*** In a postscript, dated April 24, Mr. Gill adds, that Mr. Tuther, Philosophical Instrument Maker, High Holborn, has fitted up several electrical jars in this new manner, using, however, *tin foil upon paper* for the coating around their outsides, instead of *tinselled paper*. The success has been complete; the unpleasant sensations occasioned by the passage of the electric fluid from common jars being entirely done away; whilst the most powerful effects are produced.

Though medical electricity has been administered for years in this country in nearly the same manner described in the preceding communication, yet as the method has never, so far as I know, been communicated to the public, I was unwilling to withhold the preceding letter, and trust that our medical electricians in this country will be induced by it to state the result of their experience on the subject.—T.

XVI. *On British Mathematical Periodical Works, with a Mathematical Query.*

(To Dr. Thomson.)

SIR, *London, Feb.* 10, 1819.

We have several periodical publications devoted almost exclusively to mathematical subjects, which often contain productions of considerable merit; but it is much to be regretted that the subjects are generally of a trifling nature. The questions are formed without any object beyond that of ingenious exercises; they betray no extended views, no attempts to advance physico-mathematical science, nor its application to the wants of society. The time and talents of the mathematician being consumed in the preparation of the means, he forgets the end till it be too late to consider it. How different was the course which Newton pursued!

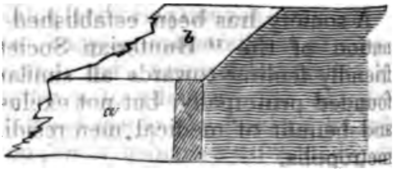
To you, who are so well aware of the imperfect state of the application of mathematical inquiry to physical science and the useful arts, I need not state that there is abundance of subjects that would not only improve the student in mathematical reasoning, but also exercise his inventive faculties in another and not less essential part of knowledge, viz. the consideration of the premises that ought to enter into the investigation of a physical problem. It is incorrectness in this that produces the paradoxical and unsatisfactory conclusions to be met with in every department of mechanical science.

While hydrodynamical science remains in its present imperfect state, there surely cannot be a want of subjects.

There are many mechanical men who know enough of science to apply the discoveries of mathematicians, and also to direct their attention to proper objects. But, unfortunately, the works to which I allude refuse to insert any question without the solution accompanies it. Hence useful subjects never come before their correspondents. An engineer might, without much difficulty, furnish them two or three hundred, all more or less useful in his profession; and I believe most of them might either be correctly solved, or approximate answers might be obtained near enough for practice. And surely the editors of such works must always be competent judges of what is fit for insertion without the caution of having the solution with the question; and they certainly would have a better opportunity of selection were this restriction removed.

Being shut out by the absurd restriction above noticed, I solicit a place for the inquiry below in your *Annals*, and not without a hope that there will speedily arrive a time when the present method of conducting mathematical works will be done away, and free scope will be given to a spirit of inquiry that will call the powers of science into a new field of action, more honourable to itself and to the enterprising minds of my countrymen.

Question.—What should be the thickness of a rectangular demirevetment so that it may be in equilibrio with the pressure of the earth, the earthen scarp above it making an angle of 45° with the horizon, and the revetment itself vertical?



In this sketch *a* is the revetment wall, and *b* the earthern scarp. The experiments of Col. Pasley prove that the common rules are not correct.* And it is necessary that every circumstance affecting the pressure of the earth be included; because the engineer, knowing the conditions of equilibrium, can better determine what will be necessary for security. I am, Sir, your obedient servant,

MASONICUS.

XVII. *Death of Hornemann.*

Baron von Zach has published an account of the death of Frederick Hornemann, a native of Hildesheim, in Lower Saxony, who was sent by the African Association in 1797 to explore the interior of Africa. Many of my readers will recollect the interesting papers published by the African Association from this enterprising traveller, and the sanguine hopes that were entertained that he would be able to penetrate to Tombuctoo. These hopes have been long extinct. The following is the account of his death communicated to Baron von Zach by Captain W. H. Smith.

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* Course of Military Instruction, vol. iii.

Captain Smith, having sojourned for some time at the court of the Dey of Tripoli, formed an acquaintance with the Bey of Fezzan, a man of much good sense, who had lately arrived from Mourzook. Among other interesting communications respecting the interior of Africa, he informed Capt. Smith that about 16 years ago he had travelled with Hornemann and his companion.* They wished to return from Tripoli to Fezzan with the design of making their way south as far as the Niger, and then to go along that river as far as Tombuctoo. But Hornemann was seized with a fever, in consequence of having drunk stagnant water in too great abundance after a very fatiguing journey. He died soon after, and was buried at Aucalus. His companion continued his journey, but fell ill at *Housca,* where he stopped in the house of a Tripoli merchant. Attempting to proceed on his journey before being completely recovered, he had a relapse, and died at Tombuctoo.

Capt. Smith adds, that he was informed by the Pasha that all the effects of Hornemann, consisting in books, manuscripts, instruments, clothes, and several large sealed letters, had been sent by the Dey of Fezzan to Tripoli to be deposited with the British consul. There is a possibility, therefore, that the researches of this enterprising but unfortunate traveller may yet be recovered.—(Jour. de Phys. lxxxvii. 474.)

XVIII. *New Medical Society.*

A society has been established in London bearing the designation of the "Hunterian Society." It professes the most friendly feeling towards all similar existing institutions, and is, founded principally, but not exclusively, for the accommodation and benefit of medical men residing in the eastern parts of the metropolis.

Its objects are to concentrate the zeal and experience of a large number of respectable practitioners whose places of residence are at a distance from professional associations already existing; and to receive and discuss communications on medical and surgical subjects. It aims particularly at the cultivation of a spirit of liberal and friendly intercourse among the members of the profession within the sphere of its influence.

It consists of honorary, corresponding, and ordinary members, and already the society is honoured by the names of a considerable number of men of character and talent.

The following is the list of the officers and council for the present year:

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* Probably Joseph Frendenboug, a Ger [illegible] Hornemann had taken into his service as an interpreter.

President.—Sir William Blizard, F.R.S.

Vice-Presidents.—James Hamilton, M.D.; George Vaux, Esq.; John Meyer, M.D.; Lewis Leese, Esq.

Treasurer.—Benjamin Robinson, M.D.

Secretaries.—John T. Conquest, M.D. F.L.S.; Thomas J. Armiger, Esq.

Council.—Thomas Addison, M.D.; Thomas Bell, Esq. F.L.S.; Henry James Cholmley, M.D.; Thomas Calloway, Esq.; William Cooke, Esq.; George Edwards, Esq.; James Alex. Gordon, M.D.; William Kingdon, Esq.; Benjamin Pierce, M.D.; James Parkinson, Esq.; Henry Richard Salmon, Esq.; Fred. Tyrrell, Esq.

The Hunterian Society holds its meetings every alternate Wednesday evening throughout the year at No. 10, St. Mary-Axe.

XIX. *Observations on the Magnetic Needle.* By Col. Beaufoy.

(To Dr. Thomson.)

MY DEAR SIR, *Bushey Heath, April 1, 1819.*

Having completed two years' observations on the daily variation of the magnetic needle, I have the pleasure to send you a table, containing the comparison of monthly observations. As every observation was made by myself, and great attention paid, I trust they have been conducted with as much accuracy as the nature of the subject admitted. It appears by the table that the variation increased from the month of April, 1817, until January, 1819; it decreased during the month of February, and increased in March the same year; consequently it remains uncertain, if the compass be yet arrived at its greatest western variation. By taking the mean of the morning differences of the two years' observations, the increase of the variation is 2′ 18″; by taking the mean difference between the noon observations, the increase is 2′ 15″; and the mean difference of the evening observations gives an augmentation of 2′ 45″; mean of the whole 2′ 25″.

Table II. contains the mean difference of the two years' observations, between the morning and noon and the noon and evening observations, whence it appears that the greatest daily variation takes place in the month of April, and the least in the month of December, the former being 11′ 48″, and the latter 4′ 07″, and that the differences in April and August are nearly the same.

As the variation of the needle appears to be a subject of general interest at present, it is my intention to continue my observations, which I trust you will as usual permit to be inserted in the *Annals of Philosophy.*

I remain, my dear Sir, yours very sincerely,

MARK BEAUFOY.

Comparison of Monthly Observations with their Differences.

TABLE I.

		1817, 1818.			1818, 1819.			Differences.	
April.	Morning.	24°	31′	52″	24°	34′	06″	+2′	14″
	Noon	24	44	43	24	44	50	+0	07
	Evening	24	35	58	24	36	36	+0	38
May.	Morning	24	32	20	24	36	18	+3	58
	Noon	24	42	35	24	45	49	+3	14
	Evening	24	34	45	24	38	35	+3	50
June.	Morning	24	31	09	24	33	47	+2	38
	Noon	24	42	14	24	45	11	+2	57
	Evening	24	34	05	24	37	40	+3	35
July.	Morning	24	31	14	24	34	24	+3	10
	Noon	24	42	06	24	44	59	+2	53
	Evening	24	35	43	24	38	14	+2	31
Aug.	Morning	24	31	16	24	34	40	+3	24
	Noon	24	42	51	24	45	58	+3	07
	Evening	24	33	45	24	37	50	+4	05
Sept.	Morning	24	33	02	24	34	29	+1	27
	Noon	24	41	36	24	45	22	+3	46
	Evening	24	34	38	24	37	28	+2	50
Oct.	Morning	24	31	06	24	35	36	+4	30
	Noon	24	40	46	24	43	28	+2	42
	Evening	—	—	—	—	—	—	—	—
Nov.	Morning	24	31	49	24	33	24	+1	35
	Noon	24	37	55	24	41	41	+3	46
	Evening	—	—	—	—	—	—	—	—
Dec.	Morning	24	34	03	24	37	04	+3	01
	Noon	24	38	02	24	41	20	+3	18
	Evening	—	—	—	—	—	—	—	—
Jan.	Morning	24	34	02	24	35	42	+1	40
	Noon	24	39	57	24	39	54	−0	03
	Evening	—	—	—	—	—	—	—	—
Feb.	Morning	24	34	22	24	34	17	−0	05
	Noon	24	40	51	24	39	55	−0	56
	Evening	—	—	—	—	—	—	—	—
March.	Morning	24	33	18	24	33	18	0	00
	Noon	24	41	37	24	41	42	+0	05
	Evening	24	33	47	24	35	17	+1	30

TABLE II.

DIFFERENCES.

Morning and Noon.			Noon and Evening.		
April	11′	48″	April	8	30″
May	9	53	May	7	32
June	11	15	June	7	50
July	10	43	July	6	34
August	11	26	August	8	34
September	9	44	September	7	26
October	8	46	October	—	—
November	7	10	November	—	—
December	4	07	December	—	—
January	5	03	January	—	—
February	6	03	February	—	—
March	7	02	March	7	08

Article IX.

Magnetical, and Meteorological Observations.

By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 42″ North. Longitude West in time 1′ 20·7″.

Magnetical Observations, 1819. — *Variation West.*

Month.		Morning Observ.		Morning Observ.			Noon Observ.		Noon Observ.			Evening Observ.		Evening Observ.		
		Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
March	1	8h	35′	24°	35′	28″	1h	55′	24°	42′	56″					
	2	—	—	—	—	—	1	15	24	40	56					
	3	8	40	24	33	11	1	15	24	41	57					
	4	8	40	24	35	56	1	25	24	43	38					
	5	8	40	24	34	40	1	25	24	39	22					
	6	8	40	24	34	30	1	15	24	43	19					
	7	8	45	24	33	12	1	35	24	42	49					
	8	8	40	24	36	37	1	25	24	40	55					
	9	8	45	24	36	10	1	25	24	41	26					
	10	8	40	24	33	09	1	25	24	39	39					
	11	8	35	24	35	42	1	15	24	41	26					
	12	8	40	24	32	38	1	25	24	40	38					
	13	8	40	24	33	38	1	25	24	42	27					
	14	8	40	24	33	36	1	20	24	41	45					
	15	8	40	24	33	34	1	10	24	43	12					
	16	8	40	24	32	16	1	20	24	39	45					
	17	8	40	24	32	52	1	20	24	41	15					
	18	8	45	24	32	54	1	25	24	42	37					
	19	8	45	24	31	19	1	30	24	40	13					
	20	8	40	24	30	11	1	30	24	40	29	6h	05′	24°	38′	18″
	21	8	40	24	31	04	1	30	24	40	03	6	05	24	36	17
	22	8	45	24	32	19	1	25	24	42	44	6	05	24	33	20
	23	8	40	24	32	01	1	20	24	42	01	6	05	24	35	00
	24	8	40	24	30	58	1	15	24	41	02	6	10	24	35	53
	25	8	40	24	32	45	1	15	24	42	21	6	10	24	33	35
	26	8	45	24	35	14	1	10	24	43	08	6	15	24	25	16
	27	8	40	24	33	00	1	25	24	43	10	—	—	—	—	—
	28	—	—	—	—	—	1	15	24	42	32	6	15	24	33	26
	29	8	40	24	31	38	1	15	24	41	58	—	—	—	—	—
	30	8	40	24	33	25	1	20	24	41	20	6	10	24	36	04
	31	8	40	24	31	58	1	15	24	41	45	6	15	24	35	37
Mean for Month.		8	41	24	33	18	1	22	24	41	42	6	09	24	35	17

In taking the monthly mean of the observations, that in the evening of the 26th is rejected, being unusually small, for which there was no apparent cause.

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
March		Inches.				Feet.		
	Morn....	28·737	34°	80°	ENE		Sleet	33°
1	Noon....	28·740	30	87	NE	—	Rain	39
	Even....	—	—	—	—		—	
	Morn....	28·840	—	97	ENE		Rain	36
2	Noon....	28·890	41	84	ENE	—	Showery	42½
	Even....	—	—	—	—		—	
	Morn....	29·082	36	65	NE by E		Cloudy	32
3	Noon....	29·152	37	55	E by N	—	Cloudy	38
	Even....	—	—	—	—		—	
	Morn....	29·432	35	53	NE		Cloudy	31½
4	Noon....	29·430	42	53	NE	—	Showery	42½
	Even....	—	—	—	—		—	
	Morn....	29·515	39	70	NE by N		Cloudy	37
5	Noon....	29·513	44	54	NE by N	—	Showery	44½
	Even....	—	—	—	—		—	
	Morn....	29·408	41	90	NE		Rain	39½
6	Noon....	29·455	47	50	NE	—	Fine	48
	Even....	—	—	—	—		—	
	Morn....	29·605	38	79	ENE		Misty	36
7	Noon....	29·610	43	65	ENE	—	Cloudy	44
	Even....	—	—	—	—		—	
	Morn....	29·654	39	70	ENE		Cloudy	37
8	Noon....	29·667	43	55	E by N	—	Fine	44
	Even....	—	—	—	—		—	
	Morn....	29·625	39	56	WNW		Cloudy	35½
9	Noon ...	29·643	46	49	Var.	—	Fine	49
	Even....	—	—	—	—		—	
	Morn....	29·634	41	82	WNW		Cloudy	38
10	Noon....	29·610	45	64	WNW	—	Cloudy	45
	Even....	—	—	—	—		—	
	Morn....	29·587	43	64	NW by W		Cloudy	37½
11	Noon....	29·607	46	55	NW by W	—	Cloudy	47
	Even....	—	—	—	—		—	
	Morn....	29·692	44	54	NW by W		Cloudy	41
12	Noon....	29·705	46	49	NW by W	—	Cloudy	48
	Even....	—	—	—	—		—	
	Morn....	29·815	43	51	Var.		Cloudy	42
13	Noon....	29·830	45	47	NW	—	Cloudy	45½
	Even....	—	—	—	—		—	
	Morn....	29·857	43	44	SE by S		Cloudy	40
14	Noon....	29·843	47	41	SSE	—	Fine	49
	Even....	—	—	—	—		—	
	Morn....	29·705	40	53	S		Very fine	34
15	Noon....	29·690	50	43	SW by S	—	Fine	53
	Even....	—	—	—	—		—	
	Morn....	29·569	47	92	W by S		Foggy	42
16	Noon....	29·575	54	50	W	—	Fine	55
	Even ...	—	—	—	—		—	
	Morn....	29·647	43	48	NW by W		Cloudy	41
17	Noon....	29·653	45	40	NW by W	—	Fine	46
	Even....	—	—	—	—		—	
	Morn....	29·758	36	44	NNE		Very fine	31
18	Noon....	29·727	44	35	W	—	Very fine	47
	Even....	—	—	—	—		—	

Meteorological Observations continued.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
March		Inches.				Feet.		
19	Morn....	29·100	45°	100°	SW		Rain	35½
	Noon....	29·043	47	39	W by N	—	Showery	48⅓
	Even	—	—	—	—		—	38
20	Morn....	28·973	41	57	NNW		Showery	
	Noon....	29·015	43	48	WNW	—	Cloudy	45
	Even....	29·116	41	52	NW		Showery	36⅓
21	Morn....	29·215	39	50	N		Clear	
	Noon....	29·330	45	37	NNW	—	Fine	46
	Even	29·332	41	37	NNW		Cloudy	36⅓
22	Morn....	29·343	40	49	W by N		Fine	
	Noon....	29·340	47	46	NW	—	Fine	50⅓
	Even	29·325	42	44	W		Cloudy	37
23	Morn....	29·276	41	55	SE by S		Cloudy	
	Noon....	29·262	48	47	S	—	Showery	48
	Even ...	29·230	44	56	S		Rain	42
24	Morn....	29·083	45	100	WSW		Showery	
	Noon....	29·113	54	36	W	—	Fine	55
	Even	29·190	48	38	W		Fine	42
25	Morn....	29·162	46	68	W by S		Cloudy	
	Noon....	29·140	51	40	W	—	Squally	51½
	Even	29·165	46	40	W		Very fine	35
26	Morn....	29·380	44	45	W		Clear	
	Noon....	29·412	52	33	W	—	Fine	52
	Even....	29·453	46	43	W by N		Showery	38
27	Morn....	29·460	46	59	SW		Cloudy	
	Noon....	29·400	48	47	WSW	—	Stormy	50
	Even	29·390	—	59	SW		Rain	45
28	Morn....	29·300	—	90	SW		Rain	
	Noon....	29·324	52	74	SW by W	—	Cloudy	51½
	Even	29·324	50	68	SW		Cloudy	46½
29	Morn....	29·266	50	56	SW		Showery	
	Noon....	29·240	54	59	WSW	—	Showery	54⅓
	Even	—	—	—	—		—	43
30	Morn....	29·505	49	65	SW by W		Cloudy	
	Noon....	29·525	53	67	W by S	—	Showery	54½
	Even	29·525	51	71	W		Showery	50
31	Morn....	29·667	52	64	W		Cloudy	
	Noon....	29·667	55	59	W	—	Cloudy	57
	Even	29·667	54	65	W by S		Cloudy	

Rain, by the pluviameter, between noon the 1st of March and noon the 1st of April 1·153 inch. The quantity that fell on the roof of my Observatory, during the same period, 1·151 inch. Evaporation between noon the 1st of March and noon the 1st of April, 2·68 inches.

ARTICLE X.

METEOROLOGICAL TABLE.

1819.	Wind.	BAROMETER.			THERMOMETER.			Hygr. at 9 a. m.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
2d Mon.										
Feb. 17	S W	29·54	29·35	29·445	52	39	45·5	64	30	☽
18	N W	29·62	29·30	29·465	49	42	45·5	90	9	
19	S W	29·85	29·15	29·500	51	31	41·0	80	—	
20	S W	29·85	28·90	29·375	52	36	44·0	69	23	
21	N	29·90	28·90	29·400	49	38	43·5	65	13	
22	Var.	29·97	29·33	29·650	45	34	39·5	67	35	
23	S E	29·53	29·33	29·430	45	28	36·5	76	27	
24	N W	29·75	29·40	29·575	45	23	34·0	67	8	●
25	N W	29·80	29·52	29·660	41	27	34·0	75		
26	S W	29·52	29·31	29·415	39	30	34·5	68	6	
27	Var.	29·31	29·25	29·280	45	37	41·0	89	7	
28	S E	29·26	29·14	29·200	40	34	37·0	78	—	
3d Mon.										
March 1	N E	29·30	29·17	29·235	41	34	37·5	87	54	
2	E	29·61	29·30	29·455	44	35	40·0	94	11	
3	N E	29·90	29·61	29·755	39	34	36·5	65		☾
4	N E	29·98	29·87	29·925	45	34	39·5	62	7	
5	N E	29·97	29·83	29·900	47	40	43·5	83		
6	N E	30·10	29·90	30·000	50	36	43·0	85		
7	N E	30·16	30·10	30·130	46	37	41·5	66		
8	N E	30·12	30·08	30·100	46	30	38·0	81		
9	S E	30·13	30·10	30·115	47	27	37·0	67		
10	N W	30·10	30·04	30·070	46	34	40·0	82		
11	N W	30·10	30·07	30·085	51	42	46·5	74		○
12	N W	30·31	30·16	30·235	51	41	46·0	65		
13	N W	30·34	30·29	30·315	48	40	44·0	61		
14	Var.	30·30	30·14	30·220	48	24	36·0	59		
15	N E	30·14	29·99	30·065	57	34	45·5	67	—	
16	W	30·13	29·98	30·055	59	40	54·5	85	—	
17	N W	30·25	30·13	30·190	46	27	36·5	60	—	
18	N W	30·15	29·45	29·800	49	35	42·0	63		
		30·34	28·90	29·768	59	23	40·78	73	2·30	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Second Month.—17. A fair day, with *Cumulostratus:* rain by night. 18. Fine and spring-like: *Cumuli* capped with *Cirrostratus,* a. m.: very stormy night. 19. After a squall in the morning, a very fine day, with large *Cumuli* and *Nimbi:* a full bright rainbow at three, p.m.: the wind settled by evening. 20. Hoar frost: very fine morning, p. m.: large ramified *Cirrus* mixed with *Cirrocumulus* at a great height: *Nimbi:* some violent wet squalls in the night from the southward. 21. Large *Cumuli,* and much wind: showers. 22. Wind shifted to N: cloudy morning: *Cumulostrati* by inosculation. 23. Wind and rain: of the latter, 0·35 in. between six and nine, and 0·27 in. more by noon: afternoon, a gale, with much cloud: evening more settled. 24. Fine morning: at noon, lofty large *Cumulostrati,* with bright sun: in the course of the afternoon, an obscurity, like the crown of the *Nimbus,* came down upon these clouds; and a considerable fall of snow took place before dark, with wind. 25. Snowy morning: the hills white with snow; which soon vanished before a bright sun, p. m. 26. *Cirrocumulus* above, while the ground and water are frozen: about half-past ten, a faint, but large *solar halo,* which continued till near 11, when obscurity came on from the southward, followed by drizzling rain, p. m. 27. Overcast morning: rain in the night. 28. Cloudy: some rain.

Third Month.—1. Snow and sleet, a. m.: a wet day. 2. Wet morning: cloudy, drizzling day. 3. A moderate easterly gale, with much cloud: a gleam of sunshine, p. m. ——18. There has been scarcely any rain since the 4th; the sky mostly grey, with light clouds; at times overcast, or filled with *Cumulostratus:* the wind northerly, breezes, and the air drying; so that the roads at the close of the period, notwithstanding some very light showers of late, remain considerably covered with dust. The diverging bars of light and shadow, produced by the sun's rays passing through the interstices of clouds, have been several times exhibited within these two days.

RESULTS.

Winds for the most part Northerly.

Barometer:	Greatest height	30·34 inches.
	Least	28·90
	Mean of the period	29·768
Thermometer:	Greatest height	59°
	Least	23
	Mean of the period	40·78
Mean of the Hygrometer		73
Evaporation		0·66 in.
Rain		2·30 inches.

Tottenham, *Third Month,* 25, 1819. L. HOWARD.

ERRATA.

Second Month.—(Feb.) 16.—In the observations on the barometer, for the figures at present in the columns, *read* max. 29·70. med. 29·535; and in the results of the barometer, the mean of the period 29·522 inches.

ANNALS

OF

PHILOSOPHY.

JUNE, 1819.

Article I.

Researches on a new Mineral Body found in the Sulphur extracted from Pyrites at Fahlun. By J. Berzelius.*

1. *Fabrication of the Sulphur at Fahlun. Phenomena exhibited when this Sulphur is employed for making Sulphuric Acid.*

PYRITES, which abound in different parts of the copper-mine, are employed at Fahlun for the preparation of sulphur. They are often mixed with galena, blende, and several other foreign bodies. They are placed on a layer of dry wood, in long horizontal furnaces, the upper part of which is covered with earth and with decomposed pyrites. The smoke passes from the furnace into horizontal chimneys, the first portion of which is constructed of brick, the last of wood. The wood is kindled below, and the heat causes the excess of sulphur to distil from the undermost stratum of pyrites. The sulphur in the state of vapour is carried off by the hot air, and afterwards deposited in the chimney in the form of flowers. When the wood is consumed, the protosulphuret of iron begins to burn, and to drive off the excess of sulphur from the stratum immediately above it. In this way the operation goes on till the pyrites are entirely burned. The powdery sulphur produced by this process contains a great deal of sulphuric acid. It is washed in water, fused and volatilized again in order to purify it. The fused sulphur before it is redistilled is a greenish [illegible] mass, with a radiated fracture; and heterogeneous subst [illegible] be seen mixed with it. The sulphur prepared during winter cannot be washed without considerable expense. It is, therefore, melted without depriving it of the acid with which it is impregnated. When the fused mass is broken and exposed for some days to the air, very acid drops exude from it, containing sulphuric acid, arsenic acid, and sulphates of iron and tin.

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* Translated from the Ann. de Chim. et Phys. ix. [illegible]

When this distilled sulphur is employed to fabricate sulphuric acid by combustion, it deposits in the bottom of the leaden chamber a reddish powder. This circumstance had been long observed by M. Bjuggren, who was possessor of the sulphuric acid manufactory at Gripsholm. He found that the deposite was not formed when any other kind of sulphur was employed; and having been informed by a chemist that the red matter must contain arsenic, he gave over employing the sulphur of Fahlun.

Since the manufactory was purchased by MM. Gahn, Eggertz, and myself, we have always burned the sulphur of Fahlun. The red sediment which formed in the liquid acid always remained at the bottom of the chamber, and had increased so much in quantity as to form a stratum about a line in thickness. The operation by which the sulphur is acidified in this manufactory differs from that which is usually employed in this respect, that the sulphur is not mixed with nitrate of potash. Flat glass plates are put at the bottom of the cistern containing nitric acid. The sulphurous acid by decomposing the nitric acid produces the nitrous gas necessary for the complete acidification of the sulphur. This modification of the process was introduced by Mr. Gustavus Schwartz, when, after the diminution of the size of the leaden chamber, the ordinary method failed entirely in producing sulphuric acid. The method of Mr. Schwartz is more expensive; but it produces a purer acid; for while we find five or six per cent. of foreign substances in English sulphuric acid, that of Gripsholm never contains more than two per cent. and that merely sulphate of lead.

In the glass vessels which contain the nitric acid, we find, after the complete decomposition of the nitric acid, a concentrated sulphuric acid, at the bottom of which is deposited a red or sometimes a brownish powder. This powder excited our attention, and induced us to examine its nature more particularly. The quantity of it, resulting from the combustion of 250 killogrammes of sulphur, did not exceed three grammes. The principal part of it was sulphur: it took fire, and burned like that body; but it left a bulky ash, which, when heated before the blow-pipe, gave out a strong smell of horseradish, analogous to that which Klaproth says is produced when tellurium is treated in the same way. After the smell ceased to be produced, there remained a metallic globule, which was merely lead. To separate the tellurium supposed to be contained in it, the reddish matter was dissolved in nitro-muriatic acid. It left a quantity of sulphur undissolved. The liquid, being mixed with a slight excess of caustic ammonia (which does not dissolve oxide of tellurium), let fall a white precipitate, which, when treated by the blow-pipe, gave out a strong smell of tellurium, and left a metallic globule of lead. The quantity of the precipitate was too small to extract tellurium from it; but we considered it in consequence of its smell of horseradish as a tellurate of lead. The liquid which had been saturated with ammonia, being evaporated to dryness, detonated, and was dissipated without any other residue than some black stains on the platinum crucible employed in the process.

2. *More particular Examination of the Substance which emitted the Smell of Horseradish when the Reddish Matter was burned. Experiments to obtain it in a separate State.*

The appearance of a substance so rare as tellurium in the sulphur of Fahlun, induced me to endeavour to obtain it in a separate state, in order to be able to form more accurate notions respecting it. I, therefore, took out the whole mass which was at the bottom of the leaden chamber. While still moist it had a reddish colour, which, on drying, became almost yellow. It weighed about 4 lbs. It was treated with nitro-muriatic acid, added in such quantity as to make the mass into a pulp; it was then digested in a moderate heat. Its colour changed by degrees; the red disappeared; and it became greenish yellow. After 48 hours' digestion, water and sulphuric acid were added, and the whole was thrown on a filter. The liquid which passed through had a deep yellow colour. The mass remaining on the filter had not sensibly diminished in bulk. It consisted chiefly of sulphur mixed with sulphate of lead and with other impurities. A small quantity of the filtered liquid was taken to find out the method of separating the substance which it was presumed to contain. This portion was precipitated by ammonia. The precipitate being well washed and dried, mixed with potassium and heated in a barometer tube, was decomposed with ignition. When put into water, a portion of it was dissolved, and the liquor assumed a strong colour of beer, very different from the wine-red colour communicated by hydrotelluret of potash; but after some hours, it became muddy, depositing red flocks, the quantity of which increased on the addition of nitric acid. This precipitate was collected, and when a part of the filter on which it was deposited was burned, it gave the circumference of the flame a blue colour, and emitted a very strong smell of horseradish. A portion of pure tellurium precipitated in the same manner from a solution of hydrotelluret of potash had a grey colour, gave a green tinge to the circumference of the flame, and emitted no perceptible odour of horseradish. On examining more closely the purified tellurium, [illegible] served in my former experiments on the oxide of tell[illegible] on telluretted hydrogen gas, I found that it produced [illegible] when exposed to the blow-pipe nor when its oxide was reduced; and that the only way of making it exhale such an odour was to heat it in a glass tube, shut by the finger, till the metal converted into vapour made a hole in the softened tube. It then burned in the hole with a blue flame, and exhaled an odour exactly similar to that of the red matter.

These experiments appeared to me to prove that the red substance could not be tellurium; but that tellurium probably contains different quantities of it, according as it has been less more purified.

As the precipitate above-mentioned was very inconsiderable, I thought that the alkaline liquor might still contain some of it. I, therefore, distilled it in a glass retort. What came over first was merely water; but after the mass began to get solid, a great quantity of a gas was disengaged, which smelled strongly of horseradish; but which was neither absorbed by water, nor by an alkaline lixivium; though it communicated its odour to the liquid through which it passed. In other respects, the gas had the properties of azote. A yellowish liquor was condensed in the receiver, which contained sulphurous acid, and was rendered muddy by a brown powder. Into the neck of the retort had sublimed a saline mass almost black; and at the bottom of it remained a small quantity of a yellowish salt which became white on cooling.

The sulphurous liquid in the receiver, being filtered and raised to the boiling temperature to drive off the sulphurous acid, became muddy again, deposited brown flocks, and lost its odour. The black salt, being treated with water, left undissolved a blackish-brown mass, analogous to that which the preceding liquor had precipitated. The solution was colourless, and contained a mixture of muriate and sulphite of ammonia.

What remained at the bottom of the retort was in a great measure dissolved by water. There remained a white powder, which was a mixture of sulphate of lead and subsulphate of tin. The dissolved portion contained bisulphate of potash (for potash had been added to the liquid to save the caustic ammonia), sulphates of iron, zinc, and copper.

The brown matter, insoluble in water, being examined more closely, was found to be the cause of the peculiar odour already mentioned; and by experiments which will be immediately related, it was found to be an elementary combustible body hitherto unknown, to which I have given the name of *selenium* (from selené, the moon), to recall its analogy with tellurium. From its chemical properties this body must be placed between sulphur and tellurium, though it has more properties in common with sulphur than with tellurium.

In the experiments made with the first portions of this body which I had obtained, I found that it could be precipitated from its acid solutions by sulphuretted hydrogen gas. I accordingly employed this reagent to separate it from the great mass of liquid which I had obtained by washing the sulphureous matter not attacked by the nitro-muriatic acid. Sulphuretted hydrogen produces a fine orange precipitate, which, towards the end, becomes a dirty yellow. The filtered hepatic liquor still contained sulphates of iron, zinc, and lime.

a. The precipitate being well washed and pressed was mixed with nitro-muriatic acid, and digested for some time. The solution at first was very rapid; but it gradually diminished. There remained an impure sulphur which could not be entirely dissolved except by reiterated digestions.

b. The acid liquor was decanted and water added to it. A copious white precipitate fell. Water was added as long as the liquid became muddy, and the whole was then thrown on the filter. The precipitate being well washed and examined by means of the blow-pipe produced at first a strong smell of horseradish. There remained a white powder, which, by means of soda and a little borax, was reduced into a metallic globule, which possessed all the properties of tin. It produced hydrogen gas when treated with muriatic acid; it was corroded but not dissolved by nitric acid, &c. The precipitate obtained, being well dried, was put into a small glass retort and heated to redness. There sublimed into the neck of the retort a matter crystallized in needles, and the oxide which remained had lost the property of giving out the smell of horseradish when treated by the blow-pipe. The sublimate had a strong acid taste; but pure, like that of muriatic or sulphuric acid, and was easily dissolved in water. It was an acid having selenium for its radical, and of which we shall examine the properties hereafter.

The liquid from which water had precipitated the seleniate of tin was mixed with muriate of barytes as long as any precipitate was produced. It was filtered, and evaporated till it began to exhale abundant vapours of muriatic acid. It was then put into a retort, and distilled to dryness. The retort was then exposed to a higher temperature. There sublimed into the upper part and neck of the retort a white substance in the form of long four-sided needles; and at the bottom of the retort remained a little white matter with red stains.

d. The sublimate was removed. It had a taste at first acid, and afterwards metallic. I considered it as a volatile nitrate or muriate with excess of acid. I took a portion of it, which I mixed with zinc filings, and distilled the mixture in a curved glass tube. Selenium sublimed without any mixture of muriate of zinc, or the disengagement of any gas. I digested the mass, which remained unsublimed in water, and the liquid, though mixed with nitrate of silver, remained clear. Of course it contained no muriatic acid. Consequently the sublimate [illegible] re selenic acid; but as it had a metallic taste of which [illegible] e- could be observed in the acid obtained by the decom the seleniate of tin, I thought it necessary to examine what could be the cause of this. I dissolved it in a little water, and added to it caustic ammonia. It was scarcely rendered muddy, and the acid which remained had a metallic taste. The same thing happened when I saturated the acid with carbonate of soda. I mixed a third portion of the acid with caustic potash; a copious, heavy, lemon-yellow precipitate fell; but the alkaline liquor still retained a little of its metallic taste.

e. The yellow precipitate did not change its colour on drying. It was volatilized before the blow-pipe. I introduced it in consequence into a glass retort, and distilled it at a red heat. It gave out at first water, and when the retort began to get red, running mercury. In the retort remained some traces of oxide of tin. The seleniate of potash being evaporated to dryness and distilled in a retort at a red heat gave still some drops of running mercury.

f. The seleniate of potash remaining at the bottom of the retort was fused: on cooling, its colour became white. The retort was broken, and the salt reduced to powder, and mixed with its own volume of sal ammoniac in powder. This mixture was introduced into a retort, and exposed to a graduated heat. Water was first disengaged containing ammonia, then traces of selenium condensed in the neck of the retort and in the receiver, and the excess of sal ammoniac began to sublime. The retort was left still some time exposed to the heat, and then withdrawn. Water being poured into the saline mass remaining in it, the salt was dissolved, leaving for residue a coarse brown powder, which was selenium reduced. It was dried and distilled in a small retort but to purify it completely and to procure it in a cohering mass. The reduction of the selenium is occasioned by the production of seleniate of ammonia, as well as by the disoxygenizing action of the hydrogen of the ammonia on the selenic acid. As selenic acid saturates more ammonia than its oxygen is capable of decomposing, there is disengaged in this operation a portion of ammonia with the azotic gas.

g. The white mass, spotted red, which remained at the bottom of the retort (in *e*), consisted chiefly of seleniate of barytes, part of which could be removed by water; of seleniate of tin; seleniate of copper; and arseniate of barytes, known by the disengagement of vapours of arsenic when treated by the blow-pipe.

It results from these experiments that the selenium in the sulphur examined is accompanied by at least seven other metallic bodies; namely, mercury, copper, tin, zinc, iron, lead, arsenic. The process for insulating the selenium will appear rather long, and I have since found methods of rendering it shorter. But I have chosen to describe it such as I practised it; because it guarantees the absence of sulphur, arsenic, and mercury. The sulphur was separated by the muriate of barytes; the arsenic remained in the state of arseniate of barytes when the selenic acid was sublimed; and the mercury was driven off by exposing the seleniate of potash to a red heat.

I have found that seleniate of potash containing sulphate of potash and oxide of mercury, when decomposed by an excess of sal ammoniac, yields pure selenium; for the sulphate of potash is not decomposed, and the oxide of mercury driving off a portion of the base of the sal ammoniac forms a double salt easily soluble in water; and the mercury is not reduced by the hydrogen of the alkali. But the selenium thus obtained often contains oxide of tin mechanically mixed, from which it may be purified by distillation. If, on the other hand, we saturate with ammonia selenic acid containing a mixture of sulphuric acid, and distil the resulting salt without the addition of fixed alkali, we obtain a selenium which contains a great deal of sulphur.

Another method, the object of which is to spare a part of the acids, is to distil the precipitate obtained by means of sulphuretted hydrogen in a glass retort. There is disengaged at first a great deal of sulphuretted hydrogen gas, then sulphur comes over containing a little selenium, and it becomes more and more impregnated with it as the process goes on. It has the colour of lead, a metallic lustre, and continues for a long time elastic. Towards the end, we obtain in the upper part of the retort a metallic substance crystallized in a confused manner. It is seleniuret of mercury proceeding from the decomposition of sulphuret of mercury by selenium at a high temperature. At the bottom of the retort remains a mixture of sulphuret of copper and sulphuret of tin.

Before discovering the method of reducing selenium by means of sal ammoniac, I tried to accomplish it by means of iron or zinc, plunging these metals into acid solutions of selenium. But this reduction is slow, incomplete, and does not yield a pure product. It was for this reason that I abandoned it.

3. *Selenium in a State of Purity.*

When selenium, after being fused, becomes solid, its surface assumes a metallic brilliancy, of a very deep brown colour, resembling polished hematites. Its fracture is conchoidal, vitreous, of the colour of lead, and perfectly metallic. If melted selenium be exposed for some time to heat, so as to cool very slowly, its surface becomes rough and granular, and of the colour of lead. The fracture is granular, dull, and looks exactly like a piece of metallic cobalt. If it be again fused and cooled rapidly, its surface becomes smooth, and its fracture vitreous as at first. Selenium has very little tendency to assume a crystallized form. When slowly separated from a solution of hydroseleniuret of ammonia, it forms on the liquid a pellicle, the upper surface of which has a pale leaden colour, and appears smooth; while the surface next the liquid has a darker colour and appears covered with small polished points. Under th microscope both surfaces present a crystalline aspect; that of the upper surface is irregular, but on the under surface it is easy to distinguish the small faces of crystals with right angles, which appear cubes or parallelopipeds. In the liquid we sometimes find the selenium deposited on the sides of the vessel below the surface of the liquid. In that case there is formed a kind of metallic vegetation, which, when viewed through a glass, appears composed of prismatic crystals, terminated in pyramids; but always too small to enable us to determine the figure with precision.

The colour of selenium varies a good deal. I have said that when rapidly cooled, its surface has a very dark brown colour, and that its fracture has the colour of lead. If by means of zinc or of sulphurous acid we precipitate it cold from a diluted solution, it assumes a cinnabar red colour; and if we boil the liquid with the precipitate, this last diminishes in bulk, and becomes almost black. If we mix an aqueous and very weak solution of selenic acid with sulphite of ammonia, or with sulphurous acid, in a glass which is only half filled with it, and expose it to the light, the sulphurous acid gradually reduces the selenium, and the liquid becomes covered with a brilliant pellicle, which, after some days, assumes the lustre of a pellicle of gold. If we remove it by a piece of paper or glass, and allow it to dry on these bodies, it resembles a pale gilding, and preserves the appearance without alteration.

The powder of selenium has a deep red colour, but it sticks together readily when pounded, and then assumes a grey colour and a smooth surface, as happens to antimony and bismuth. In very thin coats selenium is transparent, with a ruby red colour. When heated, it softens; and at 212° it is semiliquid, and melts completely at a temperature a few degrees higher. During its cooling, it retains for a long time a soft and semifluid state. Like Spanish wax it may be kneaded between the fingers and drawn out into long threads, which have a great deal of elasticity, and in which we easily perceive the transparency when they are flat and thin. These threads viewed by transmitted light are red; but by reflected light they are grey, and have the metallic lustre.

When selenium is heated in a retort, it begins to boil at a temperature below that of a red heat. It assumes the form of a dark yellow vapour, which, however, is not so intense as that of the vapour of sulphur; but it is more intense than chlorine gas. The vapour condenses in the neck of the retort and forms black drops, which unite into larger drops, as in the distillation of mercury.

If we heat selenium in the air, or in vessels so large that the vapour may be condensed by the cold air, a red smoke is formed, which has no particular smell, and which is condensed in the form of a cinnabar red powder, yielding a species of flowers, as happens to sulphur in the same circumstances. The smell of horseradish is not perceived till the heat becomes great enough to occasion oxidation.

Selenium is not a good conductor of heat. We can easily hold it between the fingers and melt it at the distance of one or two lines from the fingers without perceiving that it becomes hot. It is also a non-conductor of electricity. I put a piece of selenium an inch in length and a line in diameter in contact by one end with the conductor of an electrical machine, and by the other with a chain which was to conduct the electricity into the earth. The conductor always gave sparks at the distance of three quarters of an inch when another conducting body approached it. When I attempted to discharge a Leyden phial by the same piece of selenium, the discharge took place with a long hissing noise, and a good deal of the electricity remained in the jar. If the charge was very strong, the electricity passed in the form of a spark along the surface of the selenium; but if there was another shorter road, the spark did not follow that surface, as would have been the case if the selenium had been a conductor, as we observe with water, gilt paper, &c. But on the other hand I have not been able to render it electric by friction, at least to a degree that I could appreciate; so that selenium cannot be reckoned among idioelectrics.

It is not hard: the knife scratches it easily. It is brittle like glass, and is easily reduced to powder.

I have found its specific gravity between 4·3 and 4·32. It is difficult to take the specific gravity of it with certainty; because small cavities often occur in the middle of its mass. Slow cooling, which gives it a granular fracture, did not appear to me to alter its specific gravity.

4. *Selenium and Oxygen.*

The affinity of selenium for oxygen is not very great. If we heat it in the air without touching it with a burning body, it is usually volatilized without alteration; but if it is touched by flame it gives to its edges a fine sky-blue colour, and is volatilized with a strong smell of horseradish. The odorous substance is a gaseous oxide of selenium, which, however, I have not been able to obtain in an isolated state, and without being mixed with atmospheric air. This oxide does not appear to possess the properties of combining with acids, and of course belongs to the same class of oxides as carbonic oxide. To these I have given the name of *suboxides.*

Oxide of Selenium.—If we heat selenium in a close phial filled with common air till the greatest part of it is evaporated, the air of the phial acquires the odour of oxide of selenium in a very high degree. If we wash the air with pure water, the liquid acquires the odour of th [illegible] it as there are always formed traces of selenic acid, this [illegible] quires [illegible] perty of reddening litmus paper feebly, and of becoming muddy when mixed with sulphuretted hydrogen gas. If we remove this first water, the air still retains a great part of its smell; and if we wash it with a new quantity of water, this additional liquid assumes the smell without being precipitable by sulphuretted hydrogen gas, or giving any traces of containing an acid. Selenic oxide gas is but very little soluble in water, and does not communicate any taste to it. This oxide is likewise produced when sulphuret of selenium is dissolved in nitromuriatic acid. If the nitric acid is decomposed before all the sulphuret is decomposed, the selenic acid is in that case decomposed also, and selenium is reproduced, which precipitates in the form of a red powder, and the liquid exhales an almost insupportable odour of horseradish. If we distil together a mixture of selenium and selenic acid, there is disengaged likewise a little of this fetid gas; but the greatest part of the mixture sublimes without alteration. I have not made the experiment of passing them together through a red hot tube; in which situation the decomposition would probably be complete.

Selenic oxide gas does not combine with the caustic alkalies by the moist way; but the solutions of them assume the odour of it, as is the case with pure water.

Selenic Acid.—If we heat selenium in a large flask filled with oxygen gas, it evaporates without combustion, and the gas assumes the odour of selenic oxide, just as would have happened if the sublimation had taken place in common air; but if we heat the selenium in a glass ball of an inch in diameter, in which it has not room to volatilize and disperse; and if we allow a current of oxygen gas to pass through this ball, the selenium takes fire just when it begins to boil, and burns with a feeble flame, white towards the base, but green or greenish blue at the summit, or towards the upper edge. The oxygen gas is absorbed, and selenic acid is sublimed into the cold parts of the apparatus. The selenium is completely consumed without any residue. The excess of oxygen gas usually assumes the odour of selenic oxide.

If we pour nitric acid upon selenium and heat the mixture, the selenium dissolves with vivacity. At a low temperature this acid scarcely attacks it. If the selenium be in powder or in small fragments, these parts agglutinate together; and towards the end, when the concentrated liquid becomes boiling hot, the selenium melts, and forms black drops, which, buoyed up by the bubbles of nitrous gas attached to them, swim upon the surface of the liquid. If the liquid be now allowed to cool slowly, it deposits large prismatic crystals, longitudinally striated, which have a close resemblance to those of nitrate of potash.

Selenium dissolves still more rapidly in nitromuriatic acid: the same selenic acid is produced; and the body, by this way, cannot be united to a greater proportion of oxygen. Even when sulphuric acid, selenic acid, and peroxide of manganese, are distilled together, the selenium does not unite with an additional dose of oxygen, but oxygen gas is disengaged, and there are formed a sulphate and a seleniate of manganese.

If the nitromuriatic acid solution of selenium be evaporated in a retort, the nitric and muriatic acids come over first, and the selenic acid remains in the retort in the state of a white saline mass, which sublimes at a higher temperature. The acid does not melt; but it diminishes a little in bulk at the hottest place, and then assumes the gaseous form. Selenic acid gas has a deep yellow colour, but not so deep as that of selenium itself. Indeed it would be difficult to distinguish it from chlorine gas. I have not been able to determine the temperature at which selenic acid is converted into vapour; but if we heat a mixture of sulphuric acid and selenic acid, the latter acid sublimes first; and when it is almost completely volatilized, the sulphuric acid begins to rise.

Selenic acid condenses in the cold part of the apparatus in the form of very long four-sided needles. If the retort be large, they may have the length of two inches, or even more. If the part of the retort in which the acid is condensed be rather hot, the acid is deposited in a white, dense, semitransparent mass.

At the instant that selenic acid is taken out of the retort, it has a dry aspect and a peculiar lustre; but when left in the air, the crystals adhere to each other, and the lustre becomes dull; but they do not become liquid. The reason of this is, that the crystals combine with the water of the atmosphere, and produce, so to speak, a salt, having water for its base. A similar phenomenon takes place with vitreous boracic acid. This affinity acts with a great deal of rapidity; so that it is difficult to weigh a portion of the selenic acid without its absorbing during the operation a sufficient quantity of moisture to alter its weight. When the acid is afterwards heated, it loses its water, which distils over for a long time before the acid begins to be volatilized.

Selenic acid has a pure acid taste, which leaves a slightly burning sensation on the tongue. When in the gaseous form it has the sharp odour which usually distinguishes acids, without having any peculiar to itself. It is very soluble in cold water, and dissolves in almost every proportion in boiling water. A saturated solution of selenic acid in water crystallizes when rapidly cooled in small grains, and when slowly cooled in striated prisms. These crystals consist in a combination of selenic acid and water. The solution, when evaporated spontaneously, gives acicular crystals arranged in stars. Selenic acid dissolves with facility and in great abundance in alcoho[illegible] If we distil an alcoholic solution of it, the product has [illegible]; etherial smell, between that of nitric and sulphuric eth[illegible] igh the quantity of ether produced in my experiments [illegible] separated when the ethereal alcohol was satu[illegible] ne.

At the same time a portion of the selenium is reduced, and dry selenic acid remains in the retort, coloured red by the radical reduced. If we distil a mixture of sulphuric acid, selenic acid, and alcohol, we obtain a product having an insupportable odour, and a great deal of selenium is reduced. The very disagreeable smell of the product prevented me from examining it. We do not always obtain at each operation equal quantities from the same proportions of the materials.

Selenic Acid and Muriatic Acid.

Selenic acid seems to have no particular affinity with the acids which contain water, since it is easily separated from them by distillation without exhibiting any phenomena which would indicate combination. But it has the property of combining with anhydrous muriatic acid, producing a double acid similar to those which phosphoric acid, carbonic acid, &c. form with muriatic acid.

If we put selenium into a small glass globule formed in the middle of a barometrical tube, and introduce into it chlorine gas, this gas is absorbed by the selenium, which becomes hot, liquifies, and forms a brown coloured liquid. More chlorine gas being introduced, additional quantities of it are absorbed till the acid is converted into a white solid mass, composed of muriatic and selenic acids free from water. If we heat this double acid, it contracts a little without melting, and then evaporates in the form of a yellow vapour, just as selenic acid itself does; and condenses upon the cold part of the apparatus in the form of small white crystals. If we continue the sublimation, the sublimate becomes hot, and assumes the form of a white, dense, semifluid mass, which, on cooling, becomes filled with small cavities. It dissolves in water, with the evolution of heat, and with a kind of effervescence, occasioned by a gas which is again condensed by the liquid. The solution is colourless, limpid, and very acid.

If we mix the double anhydrous acid with selenium, the acid immediately assumes a yellow colour at the place where it is touched by the selenium; and if we apply heat, the two bodies combine and form an oily body of a brownish-yellow colour, transparent, and volatile; though a greater degree of heat is necessary to distil it over than is required by the double acid. The oily body sinks to the bottom in water, and remains for a little in a liquid state; but the water gradually decomposes it, separating from it muriatic acid and selenic acid, and leaving the selenium preserving the form of the oily body. But it is difficult to deprive it entirely of the muriatic acid: and it has always happened to me, after having pulverized the remaining selenium and digested it in boiling water, that the paper on which I dried it became friable by the influence of a portion of muriatic acid which was disengaged during the drying.

(To be continued.)

ARTICLE II.

Observations on the Measuring of the Angles of Crystals.

By M. Haüy.[*]

WHEN I composed, 20 years ago, my Traité de Mineralogie, my collection, which was not very far from its commencement, was affected by the rarity of regular and well-defined crystals among us. It was almost solely with these feeble means that I undertook to apply my theory to all the varieties hitherto described, adding those that were new to myself. It is well known that the study of such bodies requires a copious collection in order to be able to find crystals free from those accidental circumstances which alter the level of the surface and occasion perceptible differences between their inclinations and those derived from invariable laws of structure. These accidental deviations occasioned some of the inaccuracies into which I fell, notwithstanding all my care, and which I should have avoided had I been possessed of different crystals of the same variety to verify my observations. Other inaccuracies were occasioned by imperfections of which I was aware, without being able to extricate myself from the uncertainties to which they gave rise. In such cases I took care to mention that I did not guarantee the accuracy of the measurements.[†]

Such is the fate of works which constitute the foundation of a great system, especially those which result from a multitude of delicate researches. Some of them indeed exhibit the requisite degree of accuracy, but others still leave uncertainties to be cleared up, by the investigation of objects which admit more decisive conclusions.

The great increase of my collection since the publication of my treatise, has put it in my power to correct many of my old determinations. Some of these corrections have been consigned in my Tableau Comparatif; and since the publication of that book, I have continued to occupy myself with the same subject, proposing to insert the new results which I have obtained in the second edition of my Traité de Mineralogie, which I am preparing for the press.

I had no other instrument for the determination of the angles but the goniometer invented by M. Carangeot, by means of which one can scarcely hope to come nearer the truth than within half a degree, or the third of a degree when the crystal measured possesses every desirable perfection. But the method which I had adopted, and which I shall immediately explain, seemed to put it in my power to dispense with a greater degree of precision, by giving me a means of knowing from theory the term at which I ought to stop, amid the various results which I obtained sometimes on one side, and sometimes on another.

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[*] Translated from the Journal de Physique, lxxxvii. 233. (October, 1818.)

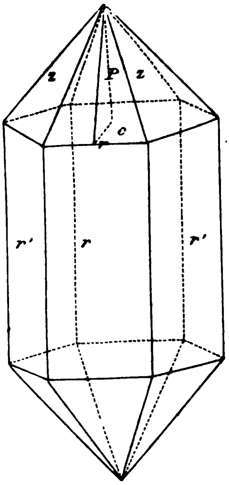
[†] When treating of the crystals of *oxide of tin* (Traité de Mineralogie, iv. 153), I employed considerations derived from the law of symmetry, which led infer a difference between the primitive form of this mineral and the cube which it does not deviate far. But the only crystals which I had (th *macles*) did not enable me to verify my notions. I pointed out the diffe my Tableau Comparatif, pp. 284 and 285.

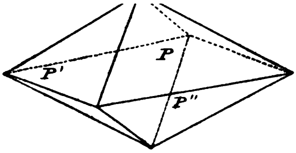
As the sciences advance, those who cultivate them invent new methods of determining with more precision the quantities which serve as data for the solution of problems. The repeating circle of Borda furnished one of these methods to astronomy and geodesy. Malus employed it to measure, by means of the angles of incidence and reflexion of light, the angles of different natural bodies, which he wished to employ for the development of his beautiful theory of double refraction. Dr. Wollaston, to whom the sciences lie under so many obligations, has contrived another very ingenious instrument, founded on the same principle expressly, for the use of crystallography. The smallness of the size, far from being a reason for excluding crystals, is, in his instrument, rather a motive of preference. This is a prerogative which this distinguished philosopher enjoys to be able to employ the method furnished by physics and chemistry to determine at one time the angles, at another the constituents of a substance, almost too small to be perceived, and which seems to borrow from the extreme dexterity of the hand who performs the experiment what it wants in bulk and weight.

Mr. Phillips, who has successfully practised the art of handling this instrument, has published in the Transactions of the Geological Society of London, the results of his measurement of the angles of a variety of crystals; and without comparing them with those which I have obtained, it is merely necessary to consider the way in which the instrument is constructed and graduated to be entitled to conclude that the ordinary goniometer is unable to contend with it, and that we have no reason to hesitate about the choice whenever we wish to obtain the requisite precision in the measurement of the angles of crystals.

The results of Mr. Phillips, who had no knowledge of most of the rectifications which I have made of my old measurements, point out very sensible differences with several of those, which seem to complete the proof of the pre-eminence of the reflecting goniometer. And the kind of disgrace into which they have a tendency to bring the one of which I made use may even be a reason for doubting if my theory be as well proved as I believed it to be, and whether it ought not even to be rejected, as not being able to exhibit in its applications that accuracy which constitutes the essence of every theory.

I propose, therefore, to show, that my theory, in the state into which it has been brought by the new attempts which I have made to complete it, cannot leave any doubt respecting





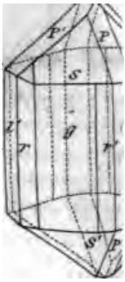
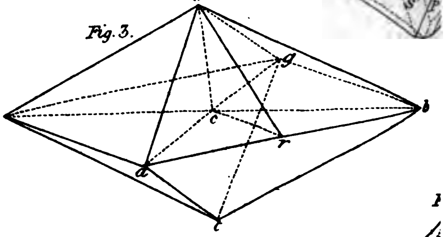


Fig. 3.



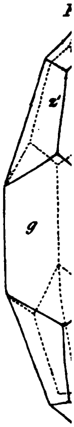


Fig. 5.

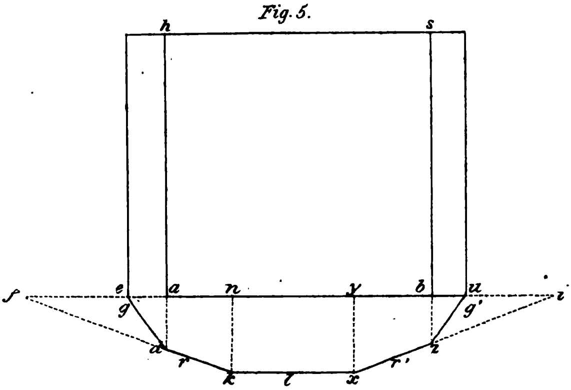


Fig. 8.

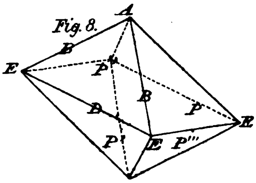


Fig. 11.

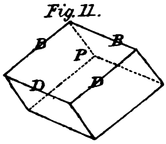


Fig. 7.

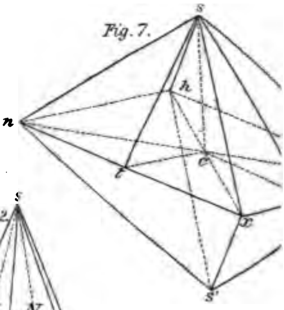


Fig. 9.

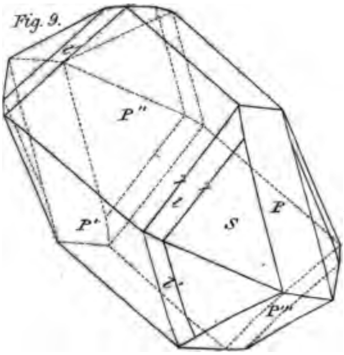


Fig. 12.

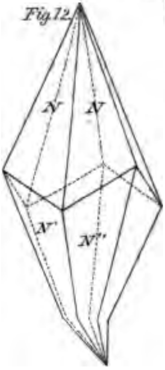
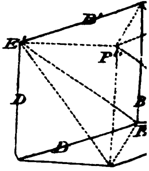


Fig.



the accuracy of the results deduced from it; that the determinations of the primitive forms on which I have fixed, lead, with regard to the secondary forms, to the true laws of decrement on which these forms depend; and that the measurements made by the reflecting goniometer itself confirm the existence of those laws.

I add that the application of the theory to the mineralogical method has likewise all the accuracy necessary to make the forms of the molecules contribute to the distinction of the species.

Finally, without excluding in certain particular cases, the use of measures obtained by the reflecting goniometer, I am convinced that those obtained by the common goniometer, which have the advantage of being at once direct and rapid, are sufficient, either to determine a new variety, or to ascertain to which of the varieties already classed in the method the crystal under examination belongs, though seen only for the first time.

I shall give three examples in support of what I have just said. The first two, namely, quartz, and oxide of tin, have been chosen from those of which the determinations have been published either in my Treatise, or my Tableau Comparatif. With the last, sulphate of lead, I have occupied myself more recently. I shall compare the results obtained by the two goniometers; and I shall draw from the comparison consequences which appear to me to guarantee the truth of all that I have advanced.

Quartz.

Though the crystals of quartz be subject to several anomalies which occasion slight variations in the position of their faces, especially of those which are parallel to the axis; yet it is not difficult to find, among the great variety deposited in collections, some possessed of all the requisite regularity for mechanical measurement. Such in particular are those called *hyacinths of compostella*, many of which are isolated and complete, and all the faces of which are smooth and perfectly level.

With respect to these crystals then, I was in a favourable situation to bring the ratio of their dimensions to a simple limit, capable of leading to results sensibly the same with those of the crystallation.

I took for a datum the inclination of one of the faces of the pyramid, such as P (fig. 1, Plate XCII.) on the adjacent face r. I found that it was between $141\frac{1}{4}°$ and $142°$. I supposed it $141\frac{3}{4}°$. On this hypothesis, if from the centre c of the base of the pyramid, of which $c\,s$ is the axis, we draw the perpendicular $c\,r$ to one of the sides, and then the line $r\,s$, we shall have $c\,r\,s = 51° \; 45'$ and $c\,r : c\,s$ :: sin. $38° \; 15'$: sin. $51° \; 45'$. To have the ratio $c\,r : c\,s$ expressed in radical quantities, I take the logarithms of the squares of these two sines, and seeking in the table of natural numbers, those to which they correspond, I obtain $c\,r : c\,s :: \sqrt{3833} : \sqrt{6167}$, or nearly $:: \sqrt{38} : \sqrt{62}$

or :: $\sqrt{19} : \sqrt{31}$. This gives us *c r s* = 51° 56′ and *c s r* = 38° 4′, the first of which is too great, and the second too small, in consequence of the quantities neglected. I see that if I add unity to each of the terms of the ratio, *c r* will be more augmented in proportion than *c s*, which has a tendency to make the two angles approach to the result given by observation. I shall then have *c r* : *c s* :: $\sqrt{20} : \sqrt{32}$, or :: $\sqrt{5} : \sqrt{8}$. Thus the ratio has all the requisite simplicity to give it the character of a limit. This ratio gives 51° 40′ for the measure of the angle *c r s*, and 141° 40′ 16″ for the incidence *l s g* or *l g d n*, results which approach very nearly to the mechanical measurement.

On the same hypothesis, the ratio of the two demi-diagonals *g* and *p* of the faces of the primitive rhomboid is that of $\sqrt{15}$ to $\sqrt{13}$; and the cosine of the angle which measures the smallest incidence of the faces of the rhomboid is $\frac{1}{13}$th of radius, which gives for that incidence 85° 36′, and for the greatest 94° 24′. Setting out from the same ratio, we have 133° 48′ 46″ for the angle which two adjacent faces on the same pyramid make with each other.

We find in the beautiful work published by Malus on double refraction a determination of the mutual incidences of the faces of a rhomboid of quartz which this celebrated philosopher ascertained by reflection making use of the repeating circle. He gives 94° 16′ for the greatest, and 85° 44′ for the smallest.*

I was curious to know how far the differences between the two measurements would go relatively to the other incidences, and what would be the ratios between the principal dimensions of the rhomboid of quartz which would result from Malus's measurement. I found, by following a method similar to that which led me to the ratio $\sqrt{5} : \sqrt{8}$, that in the present hypothesis, we should have *g* : *p* :: $\sqrt{718} : \sqrt{625}$; that the cosine of the smallest incidence of the faces would be $\frac{93}{1250}$ of radius, and that the ratio between *c r* and *c s* would be $\sqrt{1157}$ to $\sqrt{718}$. We should have for the incidence of *l g s* or *g t s* 133° 44′ 46″ instead of 133° 48′ 46″, making a difference of 4′.

We might substitute for the ratio $\sqrt{1157}$ to $\sqrt{718}$ between *c r* and *c s* that of $\sqrt{149}$ to $\sqrt{240}$, which is more simple, and which gives only half a minute of difference in the angles that depend upon it from those obtained by the first ratio.† This suggests a reflection which, I think, I ought not to omit.

If I were to show to a mathematician the ratio of $\sqrt{149}$ to $\sqrt{240}$, informing him that it is the ratio which exists in the pyramid of quartz between the perpendicular drawn from the centre of the base on one of the sides, and the length of the axis; it is very probable that after having considered it, he should find a small correction to make in it in order to transform it into another ratio much more simple. It would be necessary merely to add a single unity to the last figure of the term $\sqrt{149}$, and then the ratio, by dividing both terms by 30, would become that of $\sqrt{5}$ to $\sqrt{8}$, which is precisely that to which I arrived. I would answer that the great precision of the instrument which I employed does not allow me to alter it. He might ask me to how much the difference of the inclination * of the faces of the pyramid given by that ratio and by that resulting from the ratio $\sqrt{5}$ to $\sqrt{8}$ would amount. If I should tell him that it amounted to 4′, I doubt whether he would not be tempted to throw it rather upon the observation than to ascribe it to nature.

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* Theorie de la Double Refraction, p. 242. Mr. Phillips gives 94° 15′ and 85° 45′, which differs only 1′ from the result of Malus. It was occasioned by Mr. Phillips' goniometer being only divided into five and five minutes.

† M. Malus appears to have neglected the seconds in measuring the angles quoted in the text.

Oxide of Tin.

In the determinations which Mr. Phillips has published of the crystalline forms relative to the mineral substances, which have been the subject of the preceding articles, he has merely given the inclination of the faces of the primitive form: I have deduced from these inclinations those of the faces produced in the secondary forms by virtue of the laws of decrement, and I have compared with them those which I deduced from the ratio adopted between the principal dimensions of the primitive solid, as being a limit, the choice of which is pointed out by the character of simplicity. Mr. Phillips has been much more diffuse with respect to oxide of tin. He has measured immediately the different inclinations of the faces which terminate the secondary forms; so that here I shall have it my power to compare him with himself; and what will, I hope, render this comparison more instructive and more interesting, a part of these inclinations are independent of the primitive angles, and are derived solely from the laws of decrement whose existence cannot be called in question.

The primitive form of oxide of tin, as I have given it in my Tableau Comparatif, is an octahedron (fig. 2), in which the common base of the two pyramids of which it is composed is a square. The ratio which I have adopted between its principal dimensions is such that the oblique edge *b s* (fig. 3) of the pyramid, and the demi-diagonal *b c* of its base, are to each other as the numbers 7 to 3, which gives $\sqrt{40}$ for the value of the demi-diagonal *b c* of its base, and $\sqrt{20}$ for that of the perpendicular drawn from the centre upon one of the sides.*

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* We might have taken for the fundamental angle that which is derived from this inclination, as well as that between the faces of the rhomboid, and, in case, the instrument to agree with itself must have given immediately the 133° 44′ 46″.

Among the different angles which the faces of the crystals of oxide of tin make with each other, there is one which particularly fixed the attention of Mr. Phillips; that, namely, which measures the incidence of *s* or *g* (fig. 4) in the variety which I have called *bissexdecimale.* He was desirous to compare this incidence, such as my theory indicates, with that which the reflecting goniometer would have given: and as the instrument which he possessed was only graduated to 5′, he borrowed one of Mr. Carey, which was graduated to half minutes. The angle measured by this instrument was 133° 32′ 30″. According to my theory, it is 133° 29′ 29″. Difference 3′.

Mr. Phillips having determined all the other angles by his ordinary goniometer, I have chosen the one just mentioned in preference, to deduce from it geometrically these same angles, and to compare them with those which Mr. Phillips obtained by mechanical measurement. The ratio which I have employed is that of *c r* to *c s* (fig. 3), between the perpendicular drawn from the centre of the base of the pyramid *a h s b*, and one of the sides such as *a b*. I have found that in order to answer the end proposed, we must make $c\,r = \sqrt{702}$, and $c\,s = \sqrt{317}$.

Here a remark occurs analogous to that which I made with respect to quartz. If we multiply by 2 the two terms of the ratio, we obtain $\sqrt{1404}$ and $\sqrt{634}$. Taking away from both sides the last cypher, and then dividing by 7, we have $c\,r : c\,s :: \sqrt{20} : 3$. Now this is the ratio which I adopted.

I shall now go over the different faces of the same variety, and make a comparison of the results obtained by the different methods respecting their incidences. I shall divide them into two series, one of which will comprehend the terminal faces P, S (fig. 4), and the other the lateral faces *g*, *r*, *l*.

Terminal Faces.—We have here three kinds of results to compare; namely, 1. Those to which the theory conducts us; 2. Those determined by Mr. Phillips by means of the reflecting goniometer; 3. Those which he ought to have obtained in order to agree with himself; that is to say, those to which we are led by calculating from his own data. I shall denote these three results by the letters T, G, C.

Incidence of P on P″—T, 67° 42′ 32″; C, 67° 48′ 4″; G, 67° 50′. Diff. with T, 7′ 28″; and with C, 1′ 56″.

Incidence of P on P′—T, 133° 36′ 18″; C, 133° 32′ 38″. Diff. between T and C, 3′ 40″.—Mr. Phillips has not given the measure of this incidence.

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* It results from this determination, that the half of the square 40 of the demi-diagonal *b c* is equal to the sum 7 + 3 of the lines *b s* and *c s*.

Incidence of S on S—T, 121° 45′ 24″; C, 121° 41′ 54″; G, 121° 40′. Diff. with T, 5′ 24″; and with C, 1′ 54″.

Incidence of P on S—T, 150° 52′ 12″; C, 150° 50′ 27″; G, 150° 45′. Diff. with T, 7′ 12″; and with C, 5′ 27″.

Had not Mr. Phillips imposed upon himself the law of adhering strictly to mechanical measurements, he might have deduced the incidence of P on s from that of 121° 40′ which he had found between s and s; adding 90° to the half of this last, which would have given him 150° 50′, and would have shown him that his goniometer placed him in opposition with himself to the amount of 5′.

Lateral Faces.—The mutual incidences of these faces are in a particular case, in consequence of the common base of the two pyramids, composing the primitive octahedron, being a square. They may be assimilated to those which result from the laws of decrement on the edges of a cube, and of which it is sufficient that the measure be given, to deduce geometrically the angles derived from them with rigid accuracy.

A simple construction will make what I have said intelligible. Let $a\,b\,s\,h$ (fig. 5) be the square which represents the base indicated by the same letters, and let $d\,e$, $d\,k$, $k\,x$, &c. be lines which make with each other the same angles as the faces g, r, l (fig. 4), the letters indicating which are repeated on the lines which we are considering. Produce $k\,d$ and $x\,z$ till they meet $f\,i$; and $h\,a$, $s\,b$, till they meet $f\,k$ and $i\,x$. Then draw $k\,n$ and $x\,i$ perpendicular to $f\,i$. The triangles $a\,d\,e$, $n\,k\,f$ will be similar to those which I call *measuring triangles;* and it is by resolving them that we determine the inclinations of the faces, such as r, g (figs. 4 and 5), whose positions coincide with their exterior sides $d\,e$, $k\,d$ (fig. 5). But these triangles are obviously rectangular in the present case, and the ratios of the sides adjacent to the right angle are such that $a\,d$ is equal to $a\,e$, and that $n\,f$ is triple to $k\,n$. I add here a table of the angles to which these data lead, compared with those determined by the reflecting goniometer. I shall as before denote the former by T, and the latter by G.

Incidence of g on l and l'—T, 125°.—Mr. Phillips has omitted this incidence.

Incidence of l on r and r'—T, 161° 33′ 54″; G, 161° 35′. Diff. 1′ 6″.

Incidence of g on r and r'—T, 153° 26′ 6″; G, 153° 25′. Diff. 1′ 6″.

Incidence of r on r'—T, 143° 7′ 48″; G, 143° 10′. Diff. 2′ 12″.

Incidence of r on r'—T, 126° 52′ 12″; G, 126° 45′. Diff. 7′ 12″.

I shall further [illegible] the two faces r', r', making equal angles in contr[illegible] the face g, it is sufficient to know one of th[illegible] from it the mutual inclination of r' on r', by subtracting 90° from the known angle, and doubling the remainder. Thus the angle which one of the faces r', r' makes with g, being 153° 25′, as Mr. Phillips found it, the incidence of r on r ought to be equal to twice 153° 25′ — 90°; that is to say, to 126° 50′; and not to 126° 45′ as indicated by the reflecting goniometer. This brings it very near the true measure, which is 126° 52′ 12″.

It remains for me to speak of the variety which I call *distique*, represented in fig. 6. The faces z, z, which characterize it, result from a mixed decrement of three ranges in breadth and two in height on the lateral faces $s\,a\,h$, $s\,h\,u$ (fig. 3), from which it follows that they have two mutual different inclinations, the greatest of which is that of z on z'. These two inclinations being given, that of any of the other faces upon g is necessarily deduced from them by geometry alone.

I shall compare here also the three kinds of results obtained by the different methods, making use of the same letters as before.

Incidence of z on z'—T, 159° 6′ 58″; C, 159° 6′ 40″; G, 159° 5′. Diff. with T, 1′ 58″; with C, 1′ 40″.

Incidence of z on z—T, 118° 19′ 24″; C, 118° 18′ 22″; G, 118° 10′. Diff. with T, 9′ 24″; with C, 8′ 22″.

Incidence of z or z' on g—T, 154° 59′; C, 155° 0′ 51″; G, 155° 25′. Diff. with T, 26′; with C, 24′ 9″.

The comparison of these results leads me to a remark which does not appear to me indifferent. In those which relate to the incidence of z on z', the difference depending on the data from which we set out is reduced to a minute and some seconds; and thus the simple law of decrement, which, in my theory, determines these faces, is confirmed by the three methods. But this law being given, the other results relative either to the incidence of z on z, or to that of z on g, become corollaries from the first. So that here, as in a great variety of other cases, the crystallographer who has calculated one of these angles connected closely with a fundamental result, does not afterwards measure it, except in order to satisfy himself. He would have no doubt beforehand that observation, if exact, would agree with theory.

Yet the measures of the last two incidences by the reflecting goniometer differ more, especially the second, from the first result than all those hitherto mentioned. This is what should not have been expected; for, according to Mr. Phillips, the crystals of the *distique* variety on which he operated united at once the merit of singular beauty and of very small size, a condition so important for the precision of measurements that the same gentleman afterwards states, that crystals of a certain size, even those whose faces appear most smooth and level, presented very sensible differences in the determination of their angles; while, on the contrary, those of small size give uniform results. Hence he concludes that they are the only ones which can be depended on when precision is wanted.

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* If we denote by r the perpendicular $c\,r$ on $s\,h$ (fig. 3), by h, the axis of the pyramid, and by n the number of ranges subtracted, the ratio between the sine and cosine of the angle, which measures half the incidence of z on z' (fig. 6) is that of $\sqrt{2\,r^2\left(\frac{n-1}{n+1}\right)^2+h^2}$ to $h\,.\,\frac{n-1}{n+1}$; and with regard to half the incidence of z on z, the corresponding ratio is that of $\sqrt{r^2(n-1)^2+h^2n^2}$ to h. Further the sine of the angle, the supplement of which measures the incidence of z on g, is to the cosine as $(n-1)\sqrt{2\,r^2+h^2}$ to $(n+1)\,h$. In the present case, $n=\frac{3}{2}$; and if we make $r=\sqrt{20}$, $h=8$, we have the results indicated by T. If we make $r=\sqrt{702}$, and $h=\sqrt{317}$, we have those indicated by C. But it is visible that the angle deduced from the first ratio, being verified by observation, the measure of the angles to which the two other ratios lead is unnecessary.

I shall add here a consideration which flows naturally from what has preceded. The table of angles determined by Mr. Phillips on the different varieties of oxide of tin, by means of the reflecting goniometer, notwithstanding the superiority of that instrument over the common goniometer and the dexterity of the experimenter, presents a series of results which are really only approximations, which, having been determined independently, have no bond of union, and some of which even contradict others; far from agreeing with the simple laws of structure, they would tend to transform them into so many anomalies. If we supposed, for example, that the incidence of r on r' on the two sides of g (fig. 4) was exactly 143° 10′, as Mr. Phillips indicates, the two sides adjacent to the right angle in the measuring triangle would be to each other (confining ourselves to five figures) as 94878 : 31393, and the corresponding number of ranges subtracted would have the same ratio. Substitute for these two series the numbers 3 and 1, to which they are nearly proportional, and you have a simple law, which is that of nature. This example shows of how much consequence it is to the progress of the sciences to join theory with observation in order to regulate it, to remove the want of connexion, which would otherwise subsist between the results, and to compose a whole all the parts of which harmonize with each other.

Sulphate of Lead.

The description which I am going to give of the crystals of sulphate of lead is derived from observations made since the publication of my Tableau Comparatif. The examination of the new crystals sent me from England during that interval has enabled me to discover a considerable error contained in my former determination.* But the most decisive observations on the subject were obtained from a very interesting set of crystals sent me by M. Selb, First Counsellor of Mines to the Great Duke of Baden, and Director of the Mines of the Prince of Furstemberg. These crystals are diaphanous, of a considerable size, a regular figure, and all their faces are smooth. And the satisfaction of owing them to a philosopher so justly celebrated has doubled the value which they derive in my eyes from their perfection, and from the happy influence which they have had upon the results of my investigations.

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* This error, amounting to about 8°, was owing to the extreme rarity of crystals of sulphate [illegible] the time of my determination. To measure the primitive angles, I made [illegible] nt, in which the natural joints brought into view by mechan[illegible] unequal tissue which concealed their true inclinations.

I have continued to adopt as the type of the species, the rectangular octahedron obtained by mechanical division. But I have changed its dimensions in conformity with new measures, taken with all possible care by means of the common goniometer. Let $s\,s'$ (fig. 7) be the octahedron in question; if I draw $c\,s$, the axis of the pyramid, then $c\,r$ and $c\,t$, the one perpendicular to $k\,x$, the other to $s\,t$, then $r\,s$ and $t\,s$, the angle $s\,r\,c$ will measure half the incidence of P on P‴ (fig. 8), and the angle $s\,t\,c$ (fig. 7) half the incidence of P″ on P′. But if we make $c\,r : c\,s :: \sqrt{13} : \sqrt{8}$; and $c\,t : c\,s :: \sqrt{2} : \sqrt{3}$*, I find 76° 12′ for the first incidence, and 101° 32′ for the second. In the same hypothesis, the incidence of P on P″ (fig. 8) = 119° 51′.

The cosine of the angle which measures the incidence of the face $n\,s\,x$, (fig. 7) on the face $k\,s\,h$, situated on the opposite side of the same pyramid is $\frac{1}{5}$ radius, and that of the angle which measures the incidence of $k\,s\,x$ on $k\,s'\,x'$ is $\frac{5}{21}$ of it; so that if we represent the first cosine by $\frac{4}{20}$, it will be sufficient to add unity to each of the terms of the fraction to have the expression for the other cosine.

Mr. Phillips has found for the inclination of P on P‴ (fig. 8) the same angle, 76° 12′, as that which results from my determination. But he gives 101° 20′ instead of 101° 32′ for the incidence of P″ on P′, which makes a difference of 12′. That of P on P″, deduced from the preceding would be equal to 119° 54′; the difference of which from that which I have obtained is only 3′. The ratio of $c\,t$ to $c\,s$, which leads to the incidence of P″ on P, such as Mr. Phillips gives, it is $\sqrt{100}$ to $\sqrt{149}$. This report is similar to those in the two preceding articles: a slight modification of one of the two terms would be sufficient to reduce it to my ratio. If we add unity to the last figure of the second term, we make it $\sqrt{100} : \sqrt{150}$, or $\sqrt{2} : \sqrt{3}$, as in my determination.

According to the ratio $\sqrt{100}$ to $\sqrt{149}$, the cosine of the angle which measures the incidence of $n\,s\,x$ (fig. 7) on $k\,s\,h$ is the $\frac{49}{249}$ of the radius, instead of the $\frac{1}{5}$, which presents a kind of discordance between the two ratios, so well connected together in my determination. If this last is not the true ratio, it must be admitted at least that it is the most satisfactory to the mind.

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* If we wish to put the two ratios under a form in which they will have the line $c\,s$ for a common term, we will make $c\,s = \sqrt{24}$, $c\,t = \sqrt{16}$, $c\,r = \sqrt{39}$.

The largest of the crystals sent me by M. Selb (represented in fig. 9), exhibits the faces *s*, *l*, which I have not observed in any of the crystals from England. This induces me to give here a complete description of the variety to which this crystal belongs. I call it *decisexdecimal.*

But I must premise that, for reasons the explanation of which would carry me too far from my subject, I have adopted relatively to all the secondary forms which have an octahedron for nucleus the method indicated in my Treatise (tom. i. p. 464) for those derived from the regular octahedron. It consists in transforming the octahedron into a parallelopiped by the addition of two tetrahedrons, similar to those obtained by the mechanical division on two opposite faces of this octahedron; and in considering the decrements on which the secondary faces depend, as taking place on the edges or angles of the parallelopiped by the abstraction of one or more ranges of small parallelopipeds of the same form. The parallelopiped substituted for the octahedron in the present case, represented in fig. 10, results from the application of two tetrahedrons on the face P′ (fig. 8) and on its opposite face. After this way of viewing the crystal, the sign, representing the present variety, is

$$P\ E^4\ ({}^{4}_{3}E\ B^3\ D')\ A$$

$$P\ S\qquad l\qquad \acute{O}$$

The decrements producing the faces *l*, *s* (fig. 9) are so connected, that the intersections γ, γ, of the first with S and P″, are exactly parallel.

The following are the measures of the different angles resulting from my determination:

Incidence of	P	on P″	76°	12′
	P″	P′	101	32
	P	P″	119	51
	P	*o*	141	54
	P″	*o*	129	14
	P	*s*	154	17
	P″	*s*	141	40
	l	P″	155	15
	l	*s*′	166	25
	l	*o*	135	55

Before coming to the consequences which flow from all that precedes, I must explain the method which I followed to deduce from observation the data which enabled me to resolve the problems relative to the determination of the crystalline forms. The quantities composing the formulas, which represent generally the sides of the triangles, which I call *measuring*, express certain lines which we may conceive traced on the surface of the primitive solids, or drawn in their interior, such as the diagonals of the faces, the axes, the lines drawn perpendicular to these axes, either from the centre of the faces, or from the solid angles. If the formula, for example, relates to a rhomboid, it will contain the expressions *g* and *p*, half of the horizontal diagonal and of the oblique one of each rhomb, the expression *a* of the axis and that of the measure of the decrement to be determined, or the number of ranges subtracted, which is denoted by *n*. This last expression is always simple, or deviates very little from simplicity. With regard to the other expressions, they are equally simple in the forms, which have a particular character of symmetry and regularity, or those derived from these forms. Thus in the rhomboid which represents the subtractive molecule of the rhomboidal dodecahedron, the ratio between the semidiagonals of each rhomb is that of $\sqrt{2}$ to 1; this is also the ratio between the perpendicular drawn from the middle of each face upon the axis, and the portion of the axis which it intercepts. In the rhomboid which I consider as the subtractive molecule of the regular octahedron, the first ratio is that of 1 to $\sqrt{3}$, and the second that of 1 to $\sqrt{8}$. In the cube there exists equality between the two terms of the first ratio, and the second is that of 1 to $\sqrt{2}$. The cosine, either of the small plane angle, or of the smallest incidence of the faces, has this remarkable, that its ratio with radius is rational; and to confine myself here to that which concerns the incidence of the faces, it is half the radius in the rhomboid of garnet; it is the third in that which belongs to the regular octahedron, and in the cube it becomes zero.

A part of the laws of decrement on which the secondary varieties of the forms under consideration, are in the same case as the ratios between the dimensions of these forms; that is to say, that their measurement is considered as given *à priori*. Thus the passage of the cube to the rhomboidal dodecahedron, in *aplome*, that of the same solid to the regular octahedron in sulphuret of iron, and that of the last solid into the two preceding in fluate of lime, takes place evidently in consequence of a decrement by one range on the edges or angles of the form which performs the function of the primitive. The same consideration may be applied to the trapezoidal solid, taken as a secondary form, either of the cube as in analcime, or of the regular octahedron as in sal ammoniac, or of the rhomboidal dodecahedron as in the garnet. When the law of decrement is not indicated immediately by the aspect of the form, it may be determined with certainty from the reason of the greatest simplicity. Thus when we measure by the common goniometer the respective inclination of the pentagons of dodecahedral sulphuret of iron where their bases meet, I find it nearly equal to 127°. Further, calculation informs me that on the hypothesis that the decrements producing these pentagons take place by two rows in breadth on the edges of the primitive cube from which they set out, the inclination in question would be 126° 52′ 12″. Hence I conclude that this angle is the angle of nature; and the theory gives me the value of this small difference of 7′ 48″, which the instrument cannot determine.

When the celebrated Coulomb made his fine experiments, by means of which he demonstrated that the electric and magnetic forces followed the law of the inverse of the square of the distances, the numerical expressions of these forces, deduced from the mechanical means which he employed to measure them, never represented rigorously the law to which he supposed that these forces were subjected; but they approached it so nearly that he was authorized to consider the differences as unavoidable errors in his experiments. Thus in an experiment relative to magnetism, in which the measure of the forces depended on the square of the number of oscillations which a magnetic needle freely suspended, made in 60″, and placed successively at two different distances from the centre of a magnet, the one of which was double of the other, he observed that the corresponding number of oscillations were in the one 41, and in the other 24 and a fraction. But that the squares of these numbers, deducing the square of 15, which represented the action of the globe on the needle, should be to each other in the inverse ratio of the squares of the distance, it was necessary to suppose that the needle in its second position made 24 oscillations + $\frac{37}{100}$ very nearly. Thus calculation gave the exact value of a correction, which observation left undetermined. Such is in general the method of proceeding of the physical sciences; and we have the more reason for considering our experiments as decisive, when they give only slight differences with the results of our theories. It would be rather surprising if they agreed with them precisely.

In the species whose primitive forms differ more or less from those which I have mentioned, and which may be regarded as the limits of all the others, the ratios between the lines, which enter as data in the solution of the problems, can only be determined by observation. But I conceived, that these forms were assimilated to their limits, as the reports in question ought likewise to be simple, or at least to approach simplicity.

The method which I have adopted to obtain these ratios under the most advantageous form consists in representing under radical quantities the two terms which compose them. The result is, that among the primitive forms which belong to the different species, those which are susceptible of being cut in a certain direction, so that the section is a rhomb, possess a remarkable property, which belongs likewise to those solids which have the characters of limits; namely, that the cosine of the small angle of [illegible] rhomb is a rational number. Different rhomboidal prism [illegible] ection of which is an oblique parallelogram, in which th [illegible] are only [illegible] l two to two, possess the same property; because the line drawn from the upper extremity of the edge, on which their base originates, to the lower extremity of the opposite edge, is perpendicular to both, as I have explained in my Memoir on the Law of Symmetry.

The ratios of which we are speaking appear at intervals in the series of those which the different angles give that divide the circumference. They take place at the parts in which their component parts are susceptible of division by a common factor, which reduces their value, and frees them from the complication in which they were enveloped. The intervals which separate these ratios answer to the differences in the corresponding angles, which vary more or less, sometimes the fourth of a degree, sometimes half a degree, or more. When the crystals on which we operate have a form not very determinate, it is possible that an approaching ratio may be taken for the true one. This of necessity happened to me more than once when I was composing the geometrical part of my Treatise. I have corrected, as I have already said, a part of my old determinations, among which there are some that relate to the angles taken by Mr. Phillips, to which they approach much more nearly at present than they did formerly.

Admitting then that I have obtained, with respect to all the other species, ratios in which accuracy agrees as nearly as possible with simplicity, as, I think, has been the case in particular with regard to quartz, oxide of tin, and sulphate of lead, I consider myself as entitled to say, that these ratios are sufficient to determine without any ambiguity the laws of decrement, on which depend the secondary forms belonging to each species; for the difference in the inclination of the faces that would be produced by mistaking one law for another, would be much greater than what could exist between the angles as given by my ratio and by the reflecting goniometer. There is even in the results derived from both a convergence worthy of being remarked and very favourable to the theory. It consists in this, that the differences between the primitive angles become much less in the inclinations of the secondary faces; so that sometimes they approach so near that all difference vanishes. I shall take as an example the angles of the primitive rhomboid of calcareous spar. According to the measures of Wollaston and Malus, the angle which any face of the rhomboid forms with a parallel to the axis is 134° 37′ instead of 135° which I had indicated, from the condition that when the axis of the rhomboid was situated vertically, each of its faces was equally inclined to a vertical and a horizontal plane. If we set out from the two preceding measures, we find for the great angle which the faces of the rhomboid make with each other on the one side 105° 5′, on the other 104° 28′, which is a difference of 37′. But this difference diminishes in passing into the results of the decrements which produce the secondary forms; so that in the metastatic dodecahedron it is only 10′ and 4′ for the two respective inclinations of the faces situated towards the same summit. In another dodecahedron, which results from a decrement whose exponent is $\frac{5}{3}$ on the same edges of the primitive rhomboid, it is reduced to 2′ and 1′ 2″; and in a third dodecahedron, produced by an intermediate decrement on the lower angle, and which belongs to the variety which I have called *euthetic*, it falls between 1′ 50″ and 26″.

Now it is evident that the ordinary goniometer employed to verify these different results, is of a precision which may be considered as rigorous. The angles of the crystals of quartz, of oxide of tin, and of sulphate of lead, have presented convergences of the same nature, though rather less sensible.

I add that the forms of the integrant molecule, being the geometrical types of the species, the ratios which I have adopted have, in consequence of their simplicity, the advantage of offering neat conceptions, and easy to take up from that which characterizes these types, and the lines of demarcation between the different species deduced from them, while the mind perceives only through a mist, as it were, these distinguishing characters obscured by the great numbers in which they are enveloped.

We perceive at once and we remember the result which informs us that the cosine of the smallest incidence of the faces in the primitive rhomboid of quartz is the thirteenth of the radius. But the other result, according to which it is only the $\frac{93}{1250}$, is not easily understood, and cannot be remembered.

I have advanced above, that the ratios between the dimensions of the primitive solids, such as I have chosen them, are sufficient to determine without ambiguity the laws of decrement from which the secondary forms are derived. This I shall render sensible by an example drawn from the forms produced by decrements on the inferior edges, D, D (fig. 11), of the primitive rhomboid of calcareous spar. This decrement produces dodecahedrons with scalene triangular faces, more or less elongated, which I represent in general by that represented in fig. 12. When two ranges are abstracted, we obtain the metastatic variety in which the incidence of N on N is 144° 20′ 26″, that of N on N′ 104° 28′ 40″, and that of N on N″ 133° 26′. Among the other known dodecahedrons, that which approaches most nearly to the preceding has for its sign $\overset{\frac{1}{4}}{D}$. This law gives

For the incidence of N on N ,	139° 52′ 50″.	Diff.	4° 27′ 36″.
of N on N′,	106 13 30 .	Diff.	1 44 50 .
of N on N″,	141 12 24 .	Diff.	7 46 24 .

Hence it is obvious that we can easily avoid mistaking this last dodecahedron for the me

Let us suppose a dodec much nearer than the last, the sign of which would be $\overset{\frac{15}{2}}{D}$; we shall have for the incidence of N on N, 142° 13′ 22″, which differs from the corresponding angles of the two preceding dodecahedrons 2° 7′ 4″ and 2° 20′ 32″.

For the incidence of N on N′ 105° 15′ 14″. Diff. 36′ 34″ and 58′ 16″.

For that of N on N″, 137° 5′ 56″. Diff. 5° 14′ 30″ and 4° 6′ 28″.

We see that there remains still a certain latitude for the appreciable differences of other dodecahedrons approaching still more and more to the metastatic; but which can only be regarded as hypothetic; because the law on which they would depend would deviate more and more from the simplicity of the ordinary law, than that represented by $\overset{\frac{15}{2}}{D}$, the possibility even of which may be questioned.

I return to the measurements of angles taken by the reflecting goniometer. Mr. Phillips acknowledges that this instrument is very delicate, and requires great attention in the choice of the crystals to be measured. He mentions one which gave him successively for the inclination of two of its faces 92° 55′ and 93° 20′, or even 93° 25′, which makes a difference of 30′. He speaks of another kind of difficulty which comes from the inequalities of reflexion on the different faces. Having undertaken to determine the angles of the crystals of oxide of tin, he no doubt had at his disposal the most perfect which the county of Cornwall could furnish; and he has himself furnished the touchstone of his results, by indicating the measures, which may be considered as given *à priori*, or which depend geometrically on each other. We have seen that some of the differences which had prevented him from being of accord with himself, were equal to those which exist between the primitive angles indicated by his goniometer, and those which correspond with the limits which I have adopted, and that there is even one which goes a great deal further; namely, to 26′.

Without venturing to pretend that the simple ratios on which these limits depend are the true ratios of nature, as seems to me to have been the conclusion of philosophers of distinguished merit, I think at least that the results just stated are insufficient to demonstrate the contrary. But I will suppose, if you please, that the reflecting goniometer, employed with all the requisite skill on crystals possessed of the greatest perfection, gives appreciable differences from the angles deduced from the ratios of which I have spoken, and that these differences may amount to half a degree.

To render the new angles obtained in this way capable of being employed in the applications of the theory, we must deduce a fixed ratio between their sines and cosines. But in the first place, these angles can only be approximations; the measures from which they have been deduced having but an indefinite degree of precision. Further, suppose in the valuation of these measures we neglect every thing beyond a certain quantity, such as the minute or second, the numbers representing the sines and cosines will always exhibit a series of decimals, which has no termination; so that we must still neglect something in order to submit them to calculation. In my mode of operating the occurrence of a simple ratio, which presents itself to our view, points out the term at which we ought to stop; so that if different observers are directed by the same rule, they will agree about the choice of the fixed point in question. If, on the other hand, we suppose them to set out from measures taken with different instruments in their possession, they will necessarily vary in the choice of the limit at which they ought to remain.

Thus the measures of the angles, which have been published, though valuable in themselves, are hitherto nothing more than isolated observations, which nobody has attempted to bring under the requisite form to make suitable to the theory. It is the business of the philosophers who have given us these measures to complete their work by giving us the manner of deducing from them the fixed data for the solution of problems relative to the geometry of crystals. But I think I can affirm, that these data will do nothing more than displace a little the term from which the theory must set out, and that without any other assistance than that of the ordinary goniometer, it has at present all that is requisite to arrive at its principal object, by a route equally certain and easy.

Article III.

Memoir on Cyanogen and Hydrocyanic Acid. By M. Vauquelin.*

Prussic acid, in consequence of its singular nature, may be reckoned among the number of bodies which more particularly captivated the attention of the most celebrated chemists. The annals of the science recall the numerous experiments tried in vain by Geoffroy, Macquer, and Bergmann, to separate the colouring principle of prussian blue. It was reserved for Scheele to make that important discovery, which afterwards received from Berthollet all the development consistent with the then state of chemical science. However, the continual progress which chemistry made from day to day soon enabled us to perceive great bl[illegible]n our knowledge of the properties of prussic acid. This produced a desire to see some skilful chemist undertake this difficult task, and give it all the perfection which the great improvements in the means of analysis induced chemists to wish for and expect. This task was accordingly undertaken by M. Gay-Lussac, and the results to which he arrived would have been astonishing had they not been produced by a philosopher possessed of very uncommon sagacity; yet he acknowledged that experiments were still wanting to complete the subject. This confession occasioned the memoir of M. Vauquelin, of which we propose to give an abridgment in the present article. Even M. Vauquelin himself still admits that his own labours are far from completing our knowledge of this intricate subject. "Though I found the road struck out, and easily followed," says he, "I am yet far from pretending that I have traversed the whole of it. Many lateral paths issuing from that road still remain to be discovered. But these routs will gradually be laid open."

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* Transl[illegible]harmacie, Nov. 1818, p. 435.

Of the Alteration which Cyanogen dissolved in Water gradually undergoes.

The phenomena presented by the decomposition of cyanogen dissolved in water are very important to be known. Upon them depend the explanation of a multitude of changes observed in the reaction of this body, and of hydrocyanic acid on other bodies. This is the reason why M. Vauquelin begins with it in his memoir.

The fresh solution of cyanogen in water is quite colourless; but after an interval of some days it becomes yellow, then brown, and allows a matter of the same colour to precipitate. Then the liquor has lost the penetrating odour of cyanogen, and possesses the peculiar odour of hydrocyanic acid. If iron filings do not occasion the formation of prussian blue, as would happen if they were brought in contact with pure hydrocyanic acid, this depends upon a cause which will be understood immediately. We may, however, produce prussian blue in the liquor separated from the iron filings, by adding to it a slight excess of sulphuric acid. When, on the contrary, the iron is superabundant, the sulphuric acid combines with it by little and little, and the blue colour, which was at first manifest, disappears; but it always appears again when a new dose of acid is added.

Water seems to be the sole efficient cause of the alteration of cyanogen in the present case. M. Vauquelin has ascertained that the solution of this body in ether, though quickly and easily made, does not become coloured, and that alcohol alters it so much the less the stronger it is.

The aqueous solution of cyanogen, altered by standing, yields, when distilled, a liquid, having a strong odour of hydrocyanic acid, which contains hydriodate of ammonia and subcarbonate of ammonia. The residue of this distillation is a liquid, holding in suspension a brown matter in very minute particles. Allowed to become clear by repose and evaporated cautiously, it yields crystals which have a cooling and pungent taste, which swell and evaporate in smoke, leaving a slight trace of charcoal when thrown upon a red hot iron, but do not inflame.

They exhibit quite different phenomena from those of a nitrate when thrown upon red-hot charcoal. The aqueous solution of these crystals precipitates the nitrate of silver and acetate of lead in white flocks soluble in nitric acid. It occasions a slight muddiness in the solution of nitrate of barytes, which disappears on the addition of nitric acid. It gives out a strong odour of ammonia when triturated with caustic potash, and does not furnish prussian blue by means of sulphate of iron, not even after being mixed with potash. However, the addition of weak muriatic acid developes in it a strong odour of hydrocyanic acid, which cannot be deceitful, says M. Vauquelin, for a paper on which oxide of iron had been deposited being exposed for some time to this vapour, became blue when plunged into weak sulphuric acid.

It is evident from these facts that the crystals in question have ammonia for their base. But what is the acid which forms their other constituent? Vauquelin is of opinion that it must be a new acid hitherto unknown. The small quantity of these crystals which this celebrated chemist obtained did not put it in his power to separate this acid and to study its properties.

From the preceding facts, we may conclude that the decomposition of cyanogen dissolved in water occasions the formation of three new acids and of ammonia, which saturates them. One of these is hydrocyanic acid, and the two others are carbonic acid and the peculiar oxygenized acid just mentioned, to which M. Vauquelin has given the name of *cyanic* acid.

The brown matter deposited is owing to this, that the quantity of hydrogen requisite to produce hydrocyanic acid and ammonia does not produce a sufficient quantity of oxygen to convert all the carbon and azote of the cyanogen into an acid.

Way in which Cyanogen acts on the Metallic Oxides.

M. Vauquelin explains in this paragraph the general way in which cyanogen acts on the oxides.

This action is not the same with regard to all the oxides; but the differences have not been exactly appreciated. The alkaline oxides act with great energy on cyanogen. They make it undergo a decomposition absolutely similar to that observed with water alone; with this difference, however, that the alkalies act much more rapidly. The brown matter appears all of a sudden; but it ceases [illegible] evident when there is an excess of alkali, because this [illegible] has [illegible] the property of dissolving it. There are for[illegible] case the three acids formerly pointed out, [illegible] last substance is disengaged, because its saturating affinity is much smaller than that of the potash or soda employed in the experiment. M. Vauquelin, from these facts, concludes, that the oxides are incapable of forming *cyadides*.

Action of the red Oxide of Mercury on Cyanogen dissolved in Water.

M. Vauquelin's object in this experiment is to know if two salts are formed, and consequently two acids.

For this purpose he put the peroxide of mercury in contact with cyanogen dissolved in water. The odour of this gas speedily disappeared, the volume of the oxide diminished, the liquor acquired a mercurial taste, and the residual mercury assumed a brownish tint.

This liquor distilled in a retort gives a liquid charged with subcarbonate of ammonia, and there remain in the retort two salts which crystallize; the one in square prisms, constituting cyadide of mercury; the other in square plates, sometimes bevelled on the edges, having a taste at first cooling and pungent, but afterwards mercurial. This salt is more soluble than cyadide of mercury, and flies off in smoke when thrown upon burning coals; while the cyadide of mercury decrepitates. Muriatic acid disengages from it a strong odour of hydrocyanic acid; and if some time after we add a little potash to the mixture, a white precipitate falls, and ammonia is disengaged. In this case two salts have been formed, as happens with chlorine. But do these salts differ in the nature of their acid? or is there any other difference between them besides the existence of ammonia in one of them? Notwithstanding the probabilities in favour of the formation of two acids, M. Vauquelin does not venture to give an opinion, but leaves the point to be determined by future investigations.

Action of Hydrocyanic Acid on Hydrate of Copper.

The object of this investigation is to determine the difference between the simple and the triple prussiate.

When hydrocyanic acid is placed in contact with oxide of copper, it immediately loses its odour, and forms a compound of a greenish-yellow colour, which crystallizes in small grains. If we wash this compound with boiling water before it crystallizes, it becomes white, and dissolves in ammonia without colouring it, provided always that it is not in contact with the atmosphere. This fact had been already observed by Scheele. This prussiate of copper dissolves with effervescence in nitric acid; and M. Vauquelin is of opinion that he recognized the odour of hydrocyanic acid mixed with that of nitrous gas. When placed in contact with caustic potash, it becomes yellow, then brown, and finally slate grey.

When distilled in a tube, it gives in the first place an acid liquor, which is speedily followed by ammonia; and the brown residue, when dissolved in muriatic acid, forms a yellow solution, in which potash produces a precipitate of the same colour. M. Vauquelin does not say positively whether this prussiate be a hydrocyanate; yet as the affinity of copper for oxygen is not very strong, it is very natural to think that when the oxide of copper unites with hydrocyanic acid, it produces a cyadide. But the colour of this substance is not that of the red prussiate obtained by the action of triple prussiate of potash, or sulphate of copper. But as this prussiate contains prussian blue, it is possible that this last substance has some influence on the colour.

On the Prussiate of Copper.

The prussiate of copper is of a fine red colour. It is very bulky while moist. When treated with ammonia its volume diminishes very much. It loses its colour, becomes greenish-yellow, and assumes a crystalline form. The ammonia, in which the prussiate is digested, is scarcely tinged green; though it contains a little copper. When diluted with water and kept in a well-corked phial, it allows, after some time, a beautiful orange-coloured matter to fall down.

The prussiate of copper, rendered green by ammonia, when put in contact with water, immediately recovers its original colour, and this phenomenon may be renewed as often as we please.

M. Vauquelin concludes from these interesting facts, 1. That the common prussiate of copper is a hydrate; 2. That ammonia merely deprives it of the water which it contains; 3. That its red colour is owing to water, and that its natural colour is green.

M. Vauquelin observes that it is remarkable to see this alkali having no other action on common prussiate of copper but that of abstracting its water; while, on the other hand, it is a good solvent of the simple prussiate of copper.

Action of Cyanogen on the Oxide of Iron and on Metallic Iron.

In the researches that follow, M. Vauquelin examines a difficult question, not hitherto answered; namely, whether prussian blue be a hydrocyanate or a cyanide. This celebrated chemist having formed an opinion on the subject founded on experiment, we shall not be hereafter under the necessity of forming vague ideas respecting a substance so generally known and so useful.

From the preceding part of this article, it will be easy to see what happens when cyanogen dissolved in water is placed in contact with oxide of iron or with metallic iron. M. Vauquelin enumerates the phenomena which he observed, and he concludes from it that cyanogen, when in contact with oxide of iron, undergoes the same changes [illegible] water alone, but with greater rapidity: that ammonia, c[illegible] hydrocyanic acid, are formed; and that this last substance, instead of uniting exclusively with ammonia, combines likewise with the oxide of iron. There is also deposited a charry matter, and it is probable that cyanic acid is likewise formed; but M. Vauquelin was not able to ascertain its presence.

Cyanogen dissolved in water, when placed in contact with metallic iron, is decomposed as it would be in water alone. But in this case, the phenomena that take place are much more difficult of explanation. M. Vauquelin is led to believe that the iron decomposes the water, that it unites with the oxygen of that liquid; while the nascent hydrogen combines with a portion of the cyanogen and converts it into hydrocyanic acid; and these produce hydrocyanate of iron and ammonia. "Yet, admitting the decomposition of water," says M. Vauquelin, "we must allow at the same time that the cyanogen is likewise decomposed, as we find in the liquid carbonic acid and the peculiar acid, which could not have been formed out of the oxygen of the water. It is certain at least that metallic iron, as well as the oxide of that metal, accelerates, in a remarkable degree, the decomposition of the cyanogen, probably by acting on it as a weak alkali in proportion as it is oxidized."

Action of Hydrocyanic Acid on Iron.

The importance of this paragraph induces us to copy literally the text of M. Vauquelin.

"Hydrocyanic acid diluted with water, when placed in contact with iron in a glass vessel standing over mercury, quickly produced prussian blue, while at the same time hydrogen gas was given out. The greatest part of the prussian blue formed in that operation remains in solution in the liquid. It appears only when the liquid comes in contact with the air. This shows us that prussian blue at a minimum of oxidizement is soluble in hydrocyanic acid.

"Dry hydrocyanic acid placed in contact with iron filings undergoes no change in its colour nor smell; but the iron which becomes agglutinated together at the bottom of the vessel assumes a brown colour. After some days, the hydrocyanic acid being separated from the iron, and put in a small capsule under a glass jar, evaporated without leaving any residue. Therefore it had dissolved no iron.

"Hydrocyanic acid dissolved in water placed in contact with hydrate of iron, obtained by means of potash, and washed with boiling water, furnished prussian blue immediately without the addition of any acid. Scheele has made mention of this fact. When hydrocyanic acid is in excess on the oxide of iron, the liquor which floats over the prussian blue assumes, after some time, a beautiful purple colour. The liquor, when evaporated, leaves upon the edge of the dish circles of blue, and others of a purple colour, and likewise crystals of this last colour.

water is poured upon these substances, the purple-coloured body alone dissolves, and gives the liquid a fine purple colour. The substance which remains undissolved is prussian blue, which had been held in solution in the hydrocyanic acid. Some drops of chlorine let fall into this liquid change it to blue, and a greater quantity destroys the colour entirely. It is remarkable that potash poured into the liquid, thus deprived of its colour, occasions no precipitate whatever.

"Chemists will not fail to remark from these experiments that hydrocyanic acid does not form prussian blue directly with iron; but that on the addition of water (circumstances remaining the same) prussian blue is produced.

"They will remark likewise, that cyanogen united to water dissolves iron. This is confirmed by the inky taste which it acquires, by the disappearance of its colour, and by the residue which it leaves when evaporated; yet prussian blue is not formed.

"These first experiments seem already to show, that prussian blue is a hydrocyanate, not a cyanide.

Action of Heat on Prussian Blue.

"To complete our conviction of the nature of prussian blue, it appeared necessary to examine it with care; and in the first place I shall explain the phenomena which take place when it is dried.

"It took fire like pyrophorus, and continued to burn till it was entirely destroyed, although the platinum vessel in which it was contained was removed from the fire. During the whole time that this combustion lasted, hydrocyanate of ammonia was disengaged, as was easily ascertained by the smell. The residue was red oxide of iron.

"The ammonia and hydrocyanic acid disengaged during the whole duration of the combustion of prussian blue, give a new support to the opinion above given, that this substance is a hydrocyanate of iron.

"Prussiate of iron purified by sulphuric acid and dried as much as possible was distilled in a retort. Soon after the operation began, drops of water were seen condensed in the beak of the retort. Afterwards, when the heat had become stronger, a white vapour appeared, which condensed into needle-form crystals. The gas extricated during this operation was collected in four jars of the same size. The first portion, when mixed with a solution of potash, lost about a third of its volume. The two-thirds not absorbed burned with a blue-coloured flame, and the product of the combustion precipitated lime-water. The potash employed in this operation did not sensibly effervesce with a [illegible] but it [illegible]dered lime-water slightly milky, and it formed [illegible] blue with the acid sulphate of iron. This [illegible]rbed was chiefly hydrocyanic acid.

"The second portion of gas when agitated with water lost half of its volume, and this water had acquired very sensibly the smell and taste of hydrocyanic acid. It gave a blue colour to litmus paper reddened by an acid, and formed prussian blue with the acid sulphate of iron. It was hydrocyanate of ammonia which the water had dissolved. The gas not absorbed by water burned likewise with a blue flame, and the product of its combustion rendered lime-water milky.

"The sides of the third jar were covered with a yellow matter, which had the appearance of an oil, and which was soluble in potash. Water absorbed only a fourth part of this gas. It assumed a yellow colour, became alkaline, and acquired a very sensible taste of hydrocyanic acid. It produced a great deal of prussian blue, with acid sulphate of iron. The insoluble gas was of the same nature as in the preceding jars.

"The salt which had sublimed in the neck of the retort during the distillation of prussian blue was dissolved in water. It had a strong smell of ammonia; its solution was very alkaline; it effervesced with acids; and did not form prussian blue with acid sulphate of iron. It appears from this experiment that hydrocyanate of ammonia is more volatile than carbonate of ammonia.

"The residue of this distillation was slightly attracted by the magnet. It dissolved without effervescence in muriatic acid, and its solution was precipitated greenish-brown by ammonia. After the action of the muriatic acid there remained a small quantity of prussian blue, which had not been decomposed.

"The results furnished by the decomposition of prussian blue by heat show clearly that it contained both oxygen and hydrogen. But do these two bodies constitute an essential part of prussian blue, or do they come from the water which it still retained? This we must examine before we can form an accurate opinion respecting the nature of prussian blue.

"Without affirming that it is possible to dry prussian blue completely without partially decomposing it, we may, at least with some reason, think, that the little water which it contains cannot resist the action of the fire to the end of the decomposition of the prussian blue; the time at which the products contain the greatest proportion of oxygen and hydrogen.

"Having mutually decomposed the requisite quantities of sulphate of iron and prussiate of potash dissolved in water, I obtained a fine blue precipitate, with which I filled a flask into which I had put iron filings. The filings being agitated occasionally, the blue colour, in the course of a month, assumed a tint of green: in the course of another month, the colour became of a dirty yellowish white. When the colour seemed to undergo no further alteration, I decanted a little of it into a glass, where the colour soon became greenish; and on adding water and agitating it assumed a fine blue colour. If prussian blue were a cyanide, the only change likely to be produced by the iron filings would be to a subcyanide, and we could not conceive how this body should resume its blue colour from the contact of water and air. But it is easy to conceive how the iron could deprive the hydrate of that metal of a portion of its oxygen, and thus change the compound into a protohydrocyanate of iron. It appears even that the oxide of iron formed during this operation has not itself been separated, otherwise the prussian blue, when exposed to the air, would have assumed a greenish tint, which did not take place. Nor can we suppose that this white matter is a subhydrocyanate; because before such a compound could have been formed, hydrogen must have been disengaged, which was not the case. We might suppose indeed that the iron dividing the oxygen with that which enters into the composition of prussian blue, had formed a combination which was a subprotohydrocyanate. But had this been the case, it would have assumed a green colour when left in contact with the air.

"This ought then to induce us to conclude that prussian blue is a hydrocyanate, and that the oxygen which it furnishes during its decomposition belongs to the hydrocyanic acid and to the iron.

"If we consider the great affinity of iron for oxygen, we shall scarcely believe that at the instant of the formation of prussian blue, in consequence of the contact of hydrocyanic acid with the hydrated oxide of iron, this last substance gives up its oxygen to the hydrogen of the acid, which itself strongly retains that substance. If we attend to the decomposition of water by iron and by cyanogen itself, as has been shown above, we shall be still further from believing that prussian blue is a cyadide."

Action of Sulphuretted Hydrogen Gas on Cyanogen.

By mixing together over mercury equal volumes of cyanogen and sulphuretted hydrogen, M. Vauquelin endeavoured to ascertain whether these two gases decomposed each other. The volume remaining the same, after an interval of some days, and no perceptible change having taken place, M. Vauquelin let up a quantity of water not sufficient to dissolve the whole of the cyanogen. Immediately on the contact of the water, the gases were rapidly absorbed; the liquid assumed a yellow tint, which passed speedily to brown, and there remained merely a little azote proceeding from the decomposition of the cyanogen. The liquid had no perceptible smell, its taste was at first cooling, but became soon very bitter; and what was very remarkable, it did not sensibly redden litmus. When mixed with an acid solution of sulphate of iron, it did not produce prussian blue; but when potash was poured into the mixture, [illegible] vish-green precipitate fell, which, being redissolved by [illegible] left a little prussian blue.

The same liquor produced no action on ac sulphuret appeared till caustic potash was added; and the supernatant liquid formed a prussian blue when mixed with acid sulphate of iron. But this same liquor, which had no action on nitrate of lead, precipitated abundantly nitrate of silver and muriate of gold. In the last case only, the cyanogen did not become sensible.

What ought we to think of this liquor? asks M. Vauquelin. Is it merely a combination of cyanogen and sulphuretted hydrogen? The facts stated seem inconsistent with this opinion. Is it hydrocyanic acid holding sulphur in solution proceeding from a reciprocal decomposition of the two gases? This opinion appears more probable to M. Vauquelin. But were it true, how, he asks, is the smell of sulphur not perceptible? And how can we explain the reproduction of sulphuretted hydrogen and of cyanogen by the solution of lead to which potash is added? This difficulty is not easily got over. To throw some light upon the subject, he put hydrocyanic acid in contact with sulphur in a state of minute division; but the two bodies did not act sensibly on each other. But he does not consider this negative experiment as sufficient to overturn the preceding opinion; because the sulphur, however minutely divided, can never be in a state comparable to what it is in when thrown down from sulphuretted hydrogen gas. This liquor, whatever explanation be given of its composition, is very remarkable, from its neutral state. The class of acids has not hitherto presented any analogous compounds.

Action of the Oxide of Mercury on Triple Prussiate of Potash.

M. Vauquelin proves that by this operation a quadruple salt is formed.

The cyadide of mercury, placed in contact with potash, does not undergo any sensible change. But the case is different when the ferruginous prussiate of potash is placed in contact with the red oxide of mercury. There is precipitated a ferruginous deposit of subhydrocyanate of iron, and a part of the potash is separated, the oxide of mercury combining instead of it with the hydrocyanic acid. The new quadruple salt resulting from this singular action has properties peculiar to itself.

Action of Sulphur on Cyadide of Mercury.

M. Vauquelin endeavours to discover by this action the relative affinity of sulphur and cyanogen.

Two grammes of sulphur and as much cyadide of mercury, mixed accurately together and distilled, furnished 280 cubic centimetres of a gas, which blackened the solution of acetate of lead, and formed sulphurous acid when burned; results which demonstrate the presence of sulphuretted hydrogen in this gas.

Analysis has proved to M. Vauquelin that 110 cubic centimetres of this gas contained eight of foreign gas. This reduces to 260 centimetres the total of the cyanogen for the 280 in the preceding experiment.

In another experiment, in which only two decigrammes of sulphur for two grammes of cyadide of mercury were employed, 145 centimetres of a gas were obtained, which did not blacken the solution of acetate of lead; but which furnished sulphurous acid when burned. This result shows that cyanogen is capable of holding a little sulphur in solution.

M. Vauquelin has observed that when the mixture of cyadide of mercury and sulphur begins to get hot, a kind of explosion takes place, occasioned by the sudden disengagement of a great quantity of gas, which carries with it into the neck of the retort, and even into the receiver, a portion of sulphate of mercury. The disengagement afterwards takes place more slowly. There remains in the retort a little sulphur, cinnabar, and some metallic mercury, which could not be converted into sulphuret, probably in consequence of the great rapidity with which the gas was disengaged, carrying with it a little sulphur. M. Vauquelin never observed in the residue that charry matter which is always observed when cyadide of mercury is distilled alone.

"This experiment," observes M. Vauquelin, "proves that sulphur decomposes cyadide of mercury at a temperature much lower than that at which it is decomposed when alone. It appears to me that it would be possible in this way, by employing the requisite proportion of sulphur, to obtain pure cyanogen, without any portion of it being decomposed."

On what happens during the Solution of Cyadide of Potash in Water.

M. Vauquelin shows in this paragraph that whenever cyadide of potash is dissolved in water ammonia is formed.

A mixture of equal parts of rasped horn and subcarbonate of potash calcined at a red heat till they underwent fusion, being dissolved in water, immediately evolved ammonia. This was easily shown by suspending over the vessel litmus paper reddened by an acid. It became speedily blue. This liquor being distilled furnished a very alkaline liquid, which did not precipitate lime-water, nor form prussian blue with acid sulphate of iron. It was, therefore, pure ammonia.

We ought not to omit mentioning that during the calcination of the mixture above-mentioned, particularly towards the end of the process, M. Vauquelin observed white vapours, which had a well-characterized odour of hydrocyanic acid. Does this acid exist quite formed in the fused matter? M. Vauquelin thinks otherwise. If it were present, the matter in dissolving would not produce ammonia. He [illegible]ives that the cyanogen while passing through the moist [illegible]ere to th[illegible]gans of smell, is converted into hydrocyani[illegible]; the fo[illegible]on of which is further facilitated by a little potash which is volatilized by the action of the heat,

Observations on the Decomposition of Cyadide of Mercury by Muriatic Acid.

This paragraph contains very interesting observations, which are quite new; not only respecting what takes place during the preparation of hydrocyanic acid by the process of Gay-Lussac; but they led Vauquelin to a good method of obtaining this acid without any risk and in greater quantity than heretofore from the same weight of cyadide of mercury.

"On decomposing," says M. Vauquelin, "10 grammes of cyanide of mercury with 20 grammes of muriatic acid in an apparatus proper for condensing and collecting the hydrocyanic acid that should be disengaged; and at a temperature not sufficiently high to cause the mixture to boil, I did not observe a single trace of hydrocyanic acid. I then made it boil gently for some time; but notwithstanding this elevation of temperature, nothing appeared in the receiver which was cooled by a mixture of snow and salt. I presumed, as M. Gay-Lussac had announced, that this acid had been condensed in the part of the apparatus in which the marble was. I, therefore, heated that part; but without success. After some hours of labour, I obtained only some drops of a white liquid, having a very strong smell, which I was scarcely able to collect.

"If (judging from the composition of cyadide of mercury) all the hydrocyanic acid had been disengaged in our process, it would have amounted to two grammes and a half.

"The matter remaining in the retort ought to have been either calomel or corrosive sublimate, if things had passed as had been stated. But the crystals of the salt, which formed on the cooling of the liquid, appearing to me different from those of corrosive sublimate, I subjected them to the following trials:

"1. The salt dissolved in water much more rapidly than deutochloride of mercury, and produced a considerable degree of cold.

"2. Its solution gives with potash a white precipitate, whereas it would have given a yellow precipitate if it had been pure corrosive sublimate.

"3. A certain quantity of the salt being triturated with a solution of caustic potash, became white on the spot, and exhaled a strong odour of ammonia.

"These properties show that the salt is not corrosive sublimate, but a combination of muriate of ammonia and muriate of mercury, formerly distinguished by the name of *sal alembroth*. They show at the same time that, in the process above described, the cyanogen was in part decomposed, and that its azote united to the hydrogen of the muriatic acid, or of the water, to form ammonia, and consequently an ammoniacal mercurio-muriate. On the first supposition, charcoal must have been deposited; and on the second, carbonic acid must have been formed. But neither the one nor the other of these took place, though the liquid assumed a light-brown colour.

" M. Gay-Lussac has not spoken of this phenomenon; probably because having employed less acid it did not take place in his experiments. However in another experiment, in which I employed only 30 grammes of muriatic acid, I obtained only about two grammes of hydrocyanic acid, possessed of all the properties described by Gay-Lussac. The residue of the operation contained likewise ammoniaco-mercurial muriate, though I had conducted the process with much caution.

" It is singular that having some time afterwards repeated this process twice, I did not obtain the triple mercurial salt. I do not know to what I ought to ascribe this difference. It is possible that in the first processes, in which the apparatus was arranged the evening before, the cyadide of mercury having remained long in contact with the muriatic acid before being subjected to the action of heat, the hydrocyanic acid underwent a decomposition.

" The observations which I have just stated respecting what passes sometimes between muriatic acid and cyadide of mercury, would be of little importance unless they were to lead to a better method of obtaining that acid.

" Considering that mercury has a strong attraction for sulphur, and that cyanogen unites easily to hydrogen when presented in the proper state, I thought that sulphuretted hydrogen might be employed for decomposing dry cyadide of mercury. I operated in the following manner: I made a current of sulphuretted hydrogen gas disengaged slowly from a mixture of sulphuret of iron, and very dilute sulphuric acid pass slowly through a glass tube slightly heated, filled with cyadide of mercury, and communicating with a receiver cooled by a mixture of salt and snow.

" As soon as the sulphuretted hydrogen came in contact with the mercurial salt, this last substance blackened, and this effect gradually extended to the furthest extremity of the apparatus. During this time no trace of sulphuretted hydrogen could be perceived at the mouth of a tube proceeding from the receiver. As soon as the odour of this gas began to be perceived, the process was stopped; and the tube was heated in order to drive over the acid which might still remain in it. The apparatus being unluted, I found in the receiver a colourless fluid, which possessed all the known properties of dry prussic acid. It amounted to nearly the fifth part of the cyadide of mercury employed.

" This process is easier and f [illegible] hes more acid than the one by means of muriatic acid. [illegible] ated it several times, and always successfully. It is ne [illegible] cessary [illegible] e care to stop the process before the odour of [illegible] phu [illegible] ogen begins to be perceived, otherwise the hydrocyanic acid will be mixed with it. However, we may avoid this inconvenience by placing a little carbonate of lead at the extremity of the tube. As dry hydrocyanic acid is only required for chemical researches, and as it cannot be employed in medicine, in which that acid in a dilute state begins to be used, I think it may be worth while to bring to the recollection of apothecaries a process of M. Proust, which, perhaps, has escaped their attention. It consists in passing a current of sulphuretted hydrogen gas through a cold saturated solution of prussiate of mercury in water,* till the liquid contains an excess of it; to put the mixture into a bottle in order to agitate it from time to time, and finally to filter it.

" If the hydrocyanic acid, as almost always happens, contains traces of sulphuretted hydrogen, agitate it with a little carbonate of lead and filter it again.

" By this process we may obtain hydrocyanic acid in a much greater degree of concentration than is necessary for medicine. It has the advantage over the dry acid of being capable of being preserved a long time, always taking care to keep it as much as possible from the contact of air and heat."

Conclusions.

From the important set of experiments of which we have just given an account, M. Vauquelin draws the following conclusions:

" 1. Cyanogen dissolved in water is converted into carbonic acid, hydrocyanic acid, ammonia, and a peculiar acid, which may be called cyanic acid, and into a charry matter. This happens in consequence of the decomposition of water. These new compounds arrange themselves in the following order: The ammonia saturates the acids, producing soluble ammonical salts; while the insoluble charry matter is deposited.

" 2. The alteration produced by the alkalies, strictly so called, in the constitution of cyanogen is exactly of the same nature as the preceding; that is to say, there are formed hydrocyanic acid, carbonic acid, probably cyanic acid, charry matter, and ammonia, which is disengaged in consequence of the presence of the other alkalies. This is the reason why the solution of cyanogen in an alkali gives at once (as Gay-Lussac has observed) prussian blue with the acid solutions of iron.

" 3. The common metallic oxides produce the same effects on cyanogen dissolved in water as the alkalies, but with different degrees of rapidity, according to the affinity which each of them has for the acids developed. But in this case triple salts are formed, as we have shown when treating of oxide of iron and oxide of copper; so that cyanogen, similar in this respect to chlorine, cannot combine directly with metallic oxides; and there are formed a hydrogenated acid and oxygenized acid, because cyanogen is a compound, while chlorine is simple.

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* Experience has shown that a solution in the proportion of a gros (59·06 grs. troy) of cyadide of mercury to an ounce of water gives a hydrocyanic acid sufficiently strong to be employed in medicine. This is the strength of the acid employed by MM. Hallé, Magendie, &c.

"4. Cyanogen is capable of dissolving iron without forming prussian blue. This is proved by the fine purple colour produced in the solution by the infusion of nutgalls. But as we find prussian blue in the portion of iron not dissolved, it is not quite certain that the iron is dissolved by the cyanogen: it is more probable that it is by the cyanic acid. On this supposition the water must have been decomposed: hydrocyanic acid must have been formed which would unite with the iron, and cyanic acid which likewise dissolving iron holds it in solution. Perhaps ammonia and carbonic acid are likewise formed.

"5. Hydrocyanic acid forms prussian blue directly both with iron and its oxide without the presence either of acid or alkali; therefore, prussian blue is a hydrocyanate of iron.

"6. Whenever cyadide of potash is in contact with water, ammonia is formed, which combines with carbonic acid formed at the same time. Hence it happens, that a great quantity of cyadide of potash gives only a small quantity of hydrocyanate; a great part of it being changed into ammonia and carbonic acid.

"7. It appears to result from the preceding experiments, that the metals which, like iron, are capable of decomposing water at the ordinary temperature, form only hydrocyanates; while those incapable of decomposing that liquid form only cyadides. Among these last are silver and mercury; though mercury may possibly be an exception.

"8. Finally, all my experiments confirm the beautiful results obtained by Gay-Lussac on the composition of cyanogen and hydrocyanic acid; extending the consequences of them."

ARTICLE IV.

On Parhelia, &c. By William Burney, LL.D.

(To Dr. Thomson.)

SIR, *Gosport Observatory, Nov.* 26, 1818.

I AGAIN take the liberty of sending you some further remarks on Parhelia, to show that, with a vaporous atmosphere, they may be seen in the open day within a certain altitude, as well as early in the morning. I have been lately gratified with a sight of the Aurora Borealis, two Paraselenæ, and several meteors. Descriptions of these rare phenomena I herewith inclose for the *Annals of Philosophy*, sho[illegible] be deemed deserving of a place; and am, Sir, your o[illegible]

W. BURNEY.

Parhelia and Paraselenæ, with Solar and Lunar Halos, and their Effects; the Aurora Borealis; and coloured Meteors; seen at Gosport.

Oct. 17.—At half-past seven, a.m. a solar halo, 44° in diameter, appeared, and at its eastern edge there was a coloured Parhelion of the same altitude as the sun; a thin *Cirrostratus* was in the vicinity of, and a close corona round the sun at the time. The barometer, which had been rising, began to sink in two hours after the appearance of the Parhelion, till a shower of rain descended the next day, by the inosculation of *Cirrostratus* and *Cumuli*, thus indicating a change in the weight of the atmospheric column.

28. *Parhelia.*—From a quarter till half-past eight, a.m. two *Parhelia* appeared, each being 22° 30′ distant from, and of the same altitude as, the sun: the first Parhelion in the S.S.E. ¼ S. was remarkably bright, with the usual prismatic colours (increasing and decreasing at intervals), and apparently as large as the moon in a horizontal view, and somewhat like her full illuminated disc when rising of a golden colour over a bank of haze near the horizon. The second in the E.S.E. ½ E. progressively increased in size and colours till the first disappeared, but was not so large nor so bright, nor did it continue so long in sight: it was of the apparent size of the disc of the real sun when about 18° in altitude. The vivid red, yellow, pale blue, and silvery colours of the first, were no doubt increased from the sun being hidden, and from his direct rays being confined by a *Cirrostratus,* except at the very point of formation of the mock-sun, which just cleared the edge of that cloud, in an apparently clear but vapourous space. No solar halo was perceptible at the time; but a circular, whitish light, or corona, about 3¼° in diameter, appeared round the sun, in consequence of the vapourous state of the lower atmosphere.

Height of the barometer, 30·05 inches; of the thermometer, 53°; hygrometer of De Luc, 88°; and the wind at S.W. At nine o'clock an arched band of plumose *Cirrus* passed over to the eastward, followed immediately by an overcast sky, and some light rain fell in the afternoon. On the following day and night, the sky was completely shrouded with *Cumulostratus.*

31.—A stormy day, except two hours' sunshine in the afternoon.

Aurora Borealis.—From 11 till midnight there was a fine display of the *Aurora Borealis* between the N.N.W. and N.E. points. Some of the beams were very brilliant, and of cylindrical and conical shapes; they ascended about 28° above the northern horizon, and varied in colour, according to the density of the medium through which they passed. The horizontal light was most extensive, tending to the magnetic east and west at 36 minutes past 11. During the appearance of these coruscations, several small meteors fell almost parallel to the largest pillars of light—a circumstance much in favour of Mr. Dalton's theory of the *phenomena*. The air was serene at the time, and there were some dark longitudinal *Cirrostrati* interspersed in different parts of the sky.

Nov. 3.—At eight, a.m. a faint Parhelion appeared for a few minutes in the S.S.E. point on a *Cirrus* that was passing to a *Cirrostratus* cloud; it was 22° 30′ distant from, and of the same height as, the sun; and a small part of a halo passed through it perpendicularly. Some *Cirrus* clouds, just above the sun, were beautifully tinged with most of the prismatic colours at the time. Much *Cirrocumulus* passed over from the southward in the course of the day; and bright and dark hemispherical and pyramidal *Cumuli* appeared in different quarters. A copious dew at night, and a sinking barometer.

4, 5, and 6.—Rainy days and nights, with variable winds.

13. *Parhelia.*—At a quarter past nine, a.m. a Parhelion appeared in the S.E. by E. ¼ E. point, 22° 35′ distant from the centre of the sun's disc, whose altitude was 12° 25′ 3″. At 11 o'clock a Parhelion appeared in the S. by W. point, at the same distance as the first from the sun, which was at that time 20° 2′ 3″ high. Part of a solar halo passed through the first; but no part of one could be traced at or near the second; the vesicular vapour upon which it was formed being scarcely perceptible. These *Parhelia* were of the same altitude as the real sun, and of the apparent size of his disc; but they enlarged as their colours (red, yellow, sea-green, and pale blue) approximated to perfection. At half-past three, p.m. another Parhelion appeared in the S.W. by W. point, 23° distant from, and perpendicular to, the sun, which was 8° above the horizon: this one was situated on the top of part of a solar halo upon an attenuated *Cirrostratus* cloud; but its colours were not so well defined as those which formed the *Parhelia* above-mentioned. There was a faint corona close round the sun during their appearance; and as a proof of the vapourous state of the atmosphere, the index of the hygrometer of De Luc kept within the range of from 80° to 88° all day. A sunny day, with plumose *Cirri*, *Cirrocumuli*, and *Cirrostrati.*

Meteor.—At a quarter past seven in the evening, a low meteor moved slowly from the E. by N. to the N.E. by E. point, or through a space of about 22°, in a direction parallel to the horizon: its densest part was like the bluish colour which surrounds the wick of a lighted candle, and it left some large electric sparks behind. Light rain and wind in the night.

14.—A stormy day; a strong gale, with heavy rain from the westward at night.

15.—A continuation of the gale till noon, with sunshine, *Cirrocumuli* and *Cirrostrati.* Between six and seven, the moon rose under a semi-hal [illegible] when she had ascended 23°, an entire coloured halo [illegible] continued perfect till after midnight, having [illegible] lofty, circular, darkish canopy, suspended [illegible] iting at its extreme circumference light red, pale yellow, and green. Similar colours were perceived in a small corona, $3\frac{1}{8}$° in diameter, immediately around the moon.

Paraselenæ.—At the sides of this halo, two *Paraselenæ* appeared alternately between seven and eight o'clock; they were of the same altitude as the moon, and distant from the centre of her disc 22° 30′, thus making the halo 45° in diameter: sometimes they were faint and irregularly shaped; at other times more compact and circular, displaying the prismatic colours as in the halo, in order next to the moon. The first Paraselenæ appeared a few minutes after seven, at the edge of the halo to the right of the moon, in the eastern point, just under Aldebaran, and did not disappear entirely till near eight. The second made its appearance at half-past seven, on the left edge of the halo, diametrically opposite to the first, and was most splendid at eight, when the moon's altitude was $22\frac{1}{8}$°. When attenuated *Cirrostrati* passed over her disc, the *Paraselenæ* lost the beauty of their prismatic colours, and resembled a small portion of the galaxy seen through a clear atmosphere, but resumed them when these low vapourous clouds had cleared the halo. In addition to these mock-moons, two well-defined curved rays of light projected from the top of the halo at a quarter before eight, and drew in repeatedly and gradually like the horns of a snail: at that time the top part of the halo became very luminous, tending to produce another Paraselenæ, by the intersection of these refracted and projecting rays.

A representation of those curved luminous projections from the upper part of a circle may be made with part of a glass of water and a lighted candle, placed on a table-cloth, by giving the incident ray of light from the candle to the furthest edge of the water, an angle of from 40° to 45°; and they may be drawn in gradually by enlarging the angle of incidence, or by moving the candle slowly towards the glass, which should be a semi-circular rummer. Two-fifths of an inch of rain fell in the early part of the morning.

17.—At a quarter before nine, p. m. a meteor of the same size as that described on the 13th moved in a northerly direction.

19.—Several small meteors shot in different directions in the evening, and small *coronæ* surrounded the planets Venus, Jupiter, and Saturn, and the star Capella, in consequence of lofty haze.

25.—At nine, a. m. the trees dripped with dew, which, in the course of the night, amounted to $1\frac{75}{100}$th of an inch. The sun rose and set with a well-defined coloured halo, 44° in diameter; and in the course of the day several faint *Parhelia* appeared at its extreme edge. *Cirri* also appeared, and the intermediate modifications of clouds down to *Nimbi*, with rain in the night, followed by a rainy day.

The amount of rain here since Aug. 31, is $10\frac{1}{8}$ inches; and the quantity evaporated from an evaporator exposed to the sun and wind is $7\frac{1}{10}$th inches.

ARTICLE V.

Results of a Meteorological Journal, kept at the Observatory of the Academy, Gosport, in 1818. By W. Burney, LL D.

Lat. 50° 47′ 58″ North. Long. 1° 6′ 4″ West of Greenwich. In time, 4′ 24·3″.

Months.	COMMON BAROMETER.										DAY AND NIGHT THERMOMETER.									DE LUC'S HYGROMETER.						
	Maximum.	Minimum.	Medium.	Range of mercury.	Spaces described.	Greatest variation in 24 hours.	Number of changes.	Medium at 8 a. m.	Medium at 2 p. m.	Medium at 8 p. m.	Maximum.	Minimum.	Medium.	Range.	Greatest var. in 24 hours.	Medium at 8 a. m.	Medium at 2 p. m.	Medium at 8 a. m.	Maximum.	Minimum.	Range of Index.	Medium at 9 a. m.	Medium at 2 p. m.	Medium at 9 p. m.	Mean at 9, 2, and 9 o'clock.
	Inch.	Inch.	Inches.	Inch.	Inch.	Inch		Inch.	Inch.	Inch.															
Jan.	30·48	28·92	29·833	1·56	12·78	0·76	36	29·83	29·83	29·84	54°	25	40·80°	29·0°	17°	40·29°	46·64°	41·97°	94°	46°	48°	78·3°	72·9°	76·2°	75·8°
Feb.	30 18	28·87	29·720	1·31	8·04	0·97	25	29·72	29·71	29·71	54	23	36·50	31·0	22	36·28	45·35	37·60	96	48	48	80·2	70·1	81·1	77·1
March ..	30·36	28 50	29·653	1·86	12·44	1·00	30	29·64	29·66	29·67	58	30	41·72	28·0	22	42·29	50·74	41·74	84	43	41	66·3	64·5	66·0	65·6
April. ..	30·34	29·18	29·705	1·16	7·15	0·88	21	29·71	29·70	29·70	64	31	49·40	33·0	24	48·57	56·23	46·73	98	39	59	62·6	57·3	64·9	61·6
May	30·34	29 38	29·871	0·96	3·83	0·33	20	29·86	29·87	29·87	78	40	56·10	38·0	32	55·52	64·55	53·32	92	35	57	67·6	52·8	65·3	61·9
June	30·35	29·70	30·070	0·65	3·75	0·33	24	30·07	30·07	30·06	82	48	64·60	34·0	30	64·97	73·30	62·07	84	29	55	56·1	50·6	60·1	55·6
July	30·30	29 85	30·077	0·45	3·28	0·37	18	30·08	30·08	30·07	86	49	68·30	37·0	25	68·00	75·68	66·42	85	39	46	58·3	51·0	59·8	56·3
August ..	30·23	29·78	30 045	0·45	2·77	0·25	21	30·05	30·04	30·04	91	44	64·50	47·0	32	63·39	72·35	62·61	90	35	55	56·3	48·5	56·4	53·7
Sept.....	30·30	29·39	29·815	0·91	5·76	0·46	21	29·81	29·81	29·81	76	42	59·50	34·0	24	58·07	65·93	58·10	100	46	54	71·3	60 1	72·4	67·9
October..	30·34	29·36	29 886	0·98	3·88	0·38	20	29·88	29·88	29·89	70	40	57·70	30·0	21	54·03	61·03	54·87	90	50	40	75·0	65·7	77·0	72·5
Nov.	30·40	29·30	29 846	1·10	4·44	0·36	16	29·84	29 84	29·84	61	39	52·60	22·0	16	50 93	55·43	51·58	93	58	35	80 9	73·2	79·4	77·8
Dec.	30·60	29·40	30·054	1·20	5·48	0·47	19	30·05	30·05	30·06	55	24	41·80	31·0	18	39·32	44·45	40·29	97	59	38	78·6	72·4	78·0	76·3
	30·60	28·50	29·881	12·59	73·60	1·00	271	29·87	29·87	29·88	91	23	52·79	32·8	32	51·80	59·30	51·42	100	29	48	69·3	61·6	69·7	66·8

Months.	A SCALE OF THE WINDS.									CLOUDS.							ATMOS. PHENOMENA.									WEATHER.							
	North.	North-east.	East.	South-east.	South.	South-west.	West.	North-west.	Total number of days.	Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostratus.	Nimbus.	Anthelia.	Parhelia.	Paraselenæ.	Solar halos.	Lunar halos.	Rainbows.	Small meteors.	Lightning.	Thunder.	Clear sky.	Fair, with clouds.	Overcastsky.	Fog.	Rain.	Total number of days.	Evaporation in inches, &c.	Rain, hail, snow, in inches, &c.
Jan.	1	3	0	1	2	11	7	6	31	10	6	11	0	10	9	14	0	0	0	2	2	2	0	1	1	3	12	2	0	14	31	0·50	2·59
Feb.	2	1	4	3	4	5	5	4	28	6	7	12	1	9	7	18	0	0	0	2	2	0	0	0	0	4	8	3	3	10	28	0·80	2·76
March	2	1	1	0	4	4	15	4	31	15	7	14	0	14	6	22	0	2	0	5	5	4	1	3	3	2	14	2	0	13	31	2·35	3·80
April	1	5	9	5	4	3	2	1	30	13	9	16	1	7	9	16	0	0	0	5	4	0	0	3	2	3	14	2	0	11	30	4·50	4·63
May	3	9½	4	1½	6	2	4	1	31	14	11	15	0	11	8	11	0	0	0	2	0	0	2	0	0	6	18	3	0	4	31	7·40	0·82
June	1	4	4	1	1	7	9½	2½	30	16	9	17	2	19	11	6	0	0	0	5	2	0	0	2	1	6	18½	1	0½	4	30	9·10	0·40
July	3	1	1½	3	5	6	7	4½	31	14	10	18	0	13	9	7	0	1	0	4	0	1	8	2	3	5½	19½	2	0	4	31	8·70	0·75
August	5½	2½	6	3½	4½	1	3	5	31	9	10	20	1	14	11	2	0	3	0	4	0	0	9	1	0	4	22	3	0	2	31	8·60	0·20
Sept.	1	0½	2	0	9	4	5½	8	30	10	12	20	3	12	9	18	1	5	0	6	1	2	0	2	1	2	13	3	0	12	30	3·45	4·39
October	1½	0½	5	4½	6½	5½	5	2½	31	10	7	20	9	8	8	16	0	5	0	3	0	0	4	1	1	3	13	6	0	9	31	2·65	2·16
Nov.	0	0½	6	3	5	10	3	2½	30	11	8	19	0	10	9	17	2	6	2	3	1	1	7	0	0	0	12	6	0	12	30	1·05	4·01
Dec.	4½	9½	5	1	3½	3½	2	2	30	6	4	18	5	6	4	13	0	4	0	1	6	0	0	0	0	3	14	5	3	6	31	0·70	1·43
	25½	38	47½	26½	54½	62	68	43	365	134	100	200	22	133	100	160	3	26	2	42	23	10	31	15	12	41½	178	38	6½	101	365	49·80	72·94

ANNUAL RESULTS.

Barometer.

	Inches.
Greatest atmospheric pressure, Dec. 29. Wind N.E.	30·600
Least ditto, March 4. Wind S.W	28·500
Extreme range of the mercury	2·100
Annual mean atmospheric pressure on the barometer	29·881
Ditto at 8 a. m.	29·878
Ditto at 2 p. m.	29·878
Ditto at 8 p. m.	29·880
Greatest range of the mercury, in March	1·860
Least range of ditto, in July and August	0·450
Greatest variation in 24 hours, in March.	1·000
Least ditto, in August	0·250
Spaces described by the rising and falling of the mercury	73·600
Number of changes, caused by the variations in the weight of the atmospheric column	271

Thermometer.

	Degrees.
Greatest thermometrical heat, Aug. 6. Wind S.	91·00
Greatest cold, Feb. 9. Wind N.	23·00
Extreme range of the thermometer.	68·00
Annual mean temperature of the atmosphere	52·79
Ditto at 8 a. m.	51·80
Ditto at 2 p. m.	59·30
Ditto at 8 p. m.	51·42
Greatest range, in August	47·00
Least range, in November	22·00
Annual mean range	32·83
Greatest variation in 24 hours, in May and August	32·00
Least ditto, in November.	16·00

De Luc's Whalebone Hygrometer.

	Degrees.
Greatest humidity of the atmosphere, Sept. 5. Wind S.W.	100·0
Greatest dryness of ditto, June 8. Wind E.	29·0
Annual range of the index	71·0
Annual mean state of the hygrometer at 9 a. m.	69·3
Ditto at 9 p. m.	69·7
Ditto at 2 p. m.	61·6
Ditto at 9, 2, and 9 o'clock	66·8
Greatest mean humidity of the atmo [illegible] November.	77·8
Greatest mean dryness of ditto, in A [illegible]	53·7

A Scale of the Winds.

	Days.
From N.W. to N.	25¼
From N. to N.E	36
From N.E. to E	40¼
From E. to S.E	36¼
From S.E. to S.	54¼
From S. to S.W	62
From S.W. to W.	68
From W. to N.W	43
	365

Clouds, agreeably to the Nomenclature.

Days on which these clouds have appeared, and some of them frequently by night as well as by day.	Days.
Cirrus	157
Cirrocumulus	100
Cirrostratus	207
Stratus	22
Cumulus	153
Cumulostratus	100
Nimbus	160

State of the Weather.

	Days.
A transparent azure atmosphere without any cloud	41¼
Sun, moon, and clouds, without rain	118
An overcast sky without rain	98½
Fog	6¼
Rain, hail, snow, and sleet	101
	365

Atmospheric Phenomena.

	No. of
Anthelia, or mock-suns, nearly opposite to the sun	2
Parhelia, or mock-suns, of various prismatic colours	26
Paraselenæ, or mock-moons, of ditto	2
Solar halos, of ditto	42
Lunar halos, of ditto	28
Solar rainbows (perfect) of ditto	10
Small meteors, commonly, but improperly, called shooting stars	31
Lightning, days on which it occurred	15
Thunder, ditto	12

Evaporation.

	Inches.
Greatest quantity, in June	6·10
Least quantity, in January	0·50
Total quantity evaporated in the year	49·80

Rain, &c.

	Inches.
Greatest quantity, in April	4·63
Least quantity, in August	0·20
Total quantity that fell in the year	27·94

Beside the above-named *phenomena*, there appeared an inverted solar bow, of delicate prismatic colours to the eastward, at 6 a. m. on May 9; one perfect lunar rainbow to the westward at 45 minutes past 7, p. m. on Sept 16; and *Aurora Borealis* between the N.N.W. and N.E. points, from 11 o'clock till midnight of Oct. 31.

N.B. The barometer is hung up in the observatory, about 30 feet above high-water mark; and the self-registering horizontal day and night thermometer, and De Luc's whalebone hygrometer, are placed in an open case near a wall, in a northern aspect, out of the sun's rays, and 10 feet above the level of the garden. The pluviameter is manufactured of mixed metal, its recipient part is cylindrical, and the area of its funnel six inches square: it has a small pipe spout, with a cap at the end to prevent evaporation. Every morning at 8, a. m. after rain has fallen, it is emptied into a cylindrical glass gauge, accurately graduated to $\frac{1}{100}$th part of an inch.

The evaporator is a lead vessel, exactly of the same area, exposed with its contents to the sun and winds in dry weather. The quantity evaporated is ascertained by measuring every third day.

Both these instruments are placed clear of all obstructions on the top of the observatory, 22 feet above the level of the garden.

Barometer.

The mean atmospheric pressure on the barometer is not so great this year as last by $\frac{1}{37}$th of an inch; nor is the *maximum* height so great by $\frac{1}{25}$th of an inch. This seems contrary to what might have been expected, considering the high temperature since the middle of May, and that additional solar influence is known to raise the barometrical column in a small degree. The only rational way in which this can be accounted for is from the lowness of the barometer during the first five months, when the elasticity of the atmosphere was much disturbed by frequent gales that blew mostly between the south and west points, so as to cause the average height of the mercurial column to be $\frac{1}{8}$th of an inch below the annual mean, as shown in the table. The range of the mercury, however, is greater than it was last year by $\frac{4}{10}$ths of an inch, and as great, perhaps, as it has ever been observed in this neighbourhood.

With other common barometers, that were observed here when the *maximum* and *minimum* occurred, the annual range was $\frac{1}{15}$th of an inch greater: this, added to what has been before mentioned, makes the annual range $2\frac{1}{4}$ inches.

Horizontal Day and Night Thermometer.

The mean temperature of the extremes of heat and cold, as ascertained by this thermometer, which seems preferable for general observations to that on Six's construction, is 52·79°; that is, $2\frac{1}{4}$° higher than in the preceding year. But some doubts are entertained as to the accuracy of this way of obtaining the true mean temperature, although it has hitherto been generally adopted by those who have published their meteorological diaries from time to time for the benefit of the science. To obtain the true mean temperature of any place in Britain, four observations at least should be taken every 24 hours; namely, at eight, a.m. eight, p.m. and the *maximum* and *minimum*: the mean of these observations would approximate nearer to the truth than the mean of the extremes only.

Taking four observations each day, however irksome it may be considered, in order to bring out the annual result, is absolutely necessary, on account of the sudden transitions which are so frequently experienced by the thermometer as well as by our own feelings; and because the average of the greatest number of thermometrical observations in such a variable climate must necessarily come nearest to the truth. According to this mode of calculating, the annual mean temperature is $\frac{2}{5}$ths of a degree less than that shown in the table, as the mean of the daily extremes of heat and cold.

Those who have not got self-registering thermometers, nor time to register more than once a day, and yet wish to know the mean temperature of their own places of abode, there is reason to believe that if they were to take a daily observation at half-past eight, a.m. it would obtain the annual mean temperature nearly as correct as the mean of the daily extremes only, particularly if the situation be contiguous to the sea, and the thermometer properly shaded from the sun's rays, and in a direct northern aspect.

Comparison of the respective mean Temperatures of Gosport and Tottenham near London, for the Years 1817 and 1818.

The mean temperature of Gosport for the last two years taken together is 51·6°: of Tottenham, from the meteorological tables of Luke Howard, Esq. in the *Annals of Philosophy*, 49·4°; difference $2\frac{1}{4}$°. Now as Gosport is rather more than $\frac{2}{3}$ds of a degree further to the south than Tottenham, its mean temperature should be nearly a degree higher, because the mean temperatures of places on a direct parallel of latitude from 55° to 45° north have been found by experiments to increase nearly in the same ratio as the degree of that space of latitude. But as this discrepancy of upwards of 1½° in favour of Gosport, after allowing for its difference in latitude, is too much to be overlooked, it would appear that a solution must be sought from some local circumstances peculiar to its situation.

By comparing our monthly meteorological tables in the Naval Chronicle with those of Mr. Howard's, it appears that the thermometer here is considerably higher in the nights, also in the cold winter days, than at Tottenham; and that the diurnal summer heat at Tottenham is higher than it is here. This may be accounted for principally by a great exposure of sea that almost surrounds us, and which tends to equalize the cold in winter, and to lessen the heat in summer: and since the nocturnal temperature at Tottenham in frosty weather is from 5° to 8° below ours, the temperature of Gosport, Portsmouth, the Isle of Wight, Southampton, &c. must be more uniform, and consequently, in the case of invalids, more salubrious than in the vicinity of London. This remark holds equally good in regard to the temperature of Plymouth, and other places situate near the sea.

De Luc's Whalebone Hygrometer.

This instrument is enclosed in a brass frame 11 inches long, and graduated in a circular metallic plate 2¼ inches in diameter, from zero to 100 degrees, which, from 1000 observations, appear to be the extreme point of moisture, or complete saturation. But the index seldom advances to this point, except in long continued rains, accompanied by a south or a south-west wind, as on Sept. 5, when it reached 100 degrees. The greatest dryness it has pointed out is 29°; therefore, its annual range is 71°. In inland places where the atmosphere is naturally drier than it is here, from the influence of a great body of sea water, its range may probably amount to 75°.

The annual mean height of this instrument, which is placed near to the thermometer, is, from three observations each day, 66·8°. The mean dryness of the atmosphere, agreeably to its indications at nine, a. m. and nine, p. m. accord with each other within ⅘ths of a degree, and also with the annual mean *maximum* temperature within ⅖ths of a degree. This hygrometer points out to the observer very small changes in the humidity or dryness of the atmosphere; but it is remarkable that in summer it often does not indicate the greatest dryness for one or two hours after the *maximum* heat of the day.

Scale of the Winds.

The state of the winds was drawn up from three or more observations each day, as we [illegible] from frequent observations in the nights, according to [illegible] ise duration of each respective wind.

The winds to the ea[illegible] ou[illegible] have blown 137½ days, and *those to the* [illegible] 2[illegible] erence in favour of the latter 90 days. The most prevailing currents have been from the west, and the least from the north. The number of strong gales from particular points of the compass, or rather the days on which they have happened, are 51; namely, N. 3; N.E. 4; E. 2; S. 6; S.W. 18; W. 14; N.W. 4; beside two hurricanes from the S. and S.W. on the 4th and 7th of March. Although the vane on the observatory is somewhat higher than the neighbouring houses, yet it is seldom attended to, except the sky be cloudless, or in very dark nights. For an approximation of the prevailing winds, the direction of the low clouds, or the vane or flag at the main top-gallant-mast-head of the flag-ship in Portsmouth Harbour, is attended to; as from the eddy winds that do exist by means of local attractions and counter currents of air, it has been found impossible to ascertain by a low vane the true direction of the land breezes. Were this method generally adopted by meteorologists, and those who attend occasionally to the illustration of the weather, we should not see so many seeming contrarieties in the names assigned to the winds in regular diaries, nor to those that are said to accompany particular thunder-storms.

Nomenclature of Clouds.

The total numbers, under this head in the table, represent the number of days on which the respective modifications of clouds have appeared here. The *Cirrus* and *Cumulus* clouds are nearly equal in number in regard to the days on which they have appeared; the *Cirrocumulus* and *Cumulostratus* are exactly so; but as they have often passed over our meridian more than once or twice in a day, it would have been tedious to determine which has occurred most frequently. The *Cirrostratus* it appears has prevailed the greatest number of days, and the *Stratus* the least. The *Nimbus* has appeared on 160 different days, although it is stated in the table, under *Weather*, to have rained only 101 days. In order to reconcile this seeming difference, it is only necessary to mention that a *Nimbus* is often formed by an inosculation of the *Cumulus* and *Cirrostratus*; also by the descending *Cirrus* upon the *Cirrostratus*, and may pass off with a small portion of rain that will not measure $\frac{1}{100}$th of an inch from the rain-gauge. All the modifications of clouds seldom appear in one day; the *Stratus* is seldom seen immediately before the compound clouds are disposed to let fall their contents in rain, or otherwise: its appearance, on the contrary, is prognostic of a fair day. We have, however, by strict attention to atmospheric *phenomena*, been favoured with a sight of all of them in 12 or 14 hours, four times during the year, mostly about Michaelmas. A *sudden* simultaneous change of temperature and pressure of the electric state of the atmosphere, and of reverse winds, is sometimes the cause of this anomaly.

As most of the atmospheric phenomena have been explained in the *Annals*, it will not now be necessary to enter on any explanation of them.

Evaporation.

The quantity that has evaporated here this year is beyond all former observations; indeed, it is 15½ inches more than in 1817, and double that of 1816. In June last it amounted to 9$\frac{1}{10}$th inches; and from the 3d to the 9th of that month, the lead evaporator, six inches square, exposed to the weather, actually lost half an inch regularly every 24 hours, with easterly and north-easterly winds, and with a mean temperature of 64·7°. Under all the circumstances that attended this evaporation, a pond of water 15 inches deep and of any square area, would, in 30 days (taking day and night together), be entirely dried up. Hence when we see or hear of springs being partly dried up in summer, after a long drought, accompanied by a high temperature, our astonishment ceases. In April, May, June, July, August, and September, the quantity that evaporated was 41¾ inches. In October, November, December, January, February, and March, it amounted only to eight inches; so that the evaporation in the spring and summer months was five times more than in the months of autumn and winter. In the spring and summer of 1817, it was four times as much as in autumn and winter of that year.

Rain, &c.

The quantity of rain, as might have been expected from so dry a summer, is 2·63 inches less than in the preceding year. The mean of the last three years' rain here is 30·35 inches; and the mean of the last two years' rain at Tottenham is 25·39 inches; the mean difference for several years is about four inches a year more at Gosport than at Tottenham and its neighbourhood. But Tottenham has less attractions of the lower *strata* of clouds, in regard to sea, hills, &c. than Gosport, which may account for the difference in the average quantity of rain.

Comparison of the Evaporation, and the Quantity of Rain at Gosport, Bushey Heath, near Stanmore, and Tottenham, in the Year 1818, viz.

	Evaporation.	Rain.
At Gosport	49·800 in.	27·940 in.
At Bushey Heath	40·025	21·405
At Tottenham	24·950	25·950

We know not how to account for the comparative difference that appears in the evaporation at the latter place, unless the evaporator there is partly sheltered from the free and combined action of the sunshine and winds.

Variation of the M[illegible]etic Needle.

From about 100 [illegible] observations, with a good magnetic needle prepared for the purpose, the mean variation here has been found to be $24\frac{1}{4}^{\circ}$ west; and from observations made with the same needle in the early part of the year, there is reason to conclude that the variation westward has not attained its *maximum*, but that it is still increasing very slowly; yet it has been mentioned in late publications in this country, as well as in Paris, that the magnetic needle is receding from its western limits.

It is necessary to observe that no correct conclusion can be drawn from two or three cursory observations, however perfect the apparatus may be; and that a regular series must be entered on to determine the question with any degree of accuracy. At the time of the equinoxes, the magnetic needle is often disturbed by strong electric winds; and at the close of summer we have observed that the needle generally indicates the greatest variation: therefore, a month before and a month after the summer and winter solstices, seem the most proper times to commence a series of observations to determine the true mean state of the magnetic needle westward.

Article VI.

Analyses of Books.

Recherches sur l'Identité des Forces Chimiques et Electriques. Par M. H. C. Œrsted, Professeur à l'Université Royale de Copenhagne, et Membre de la Societé Royale des Sciences de la même Ville, &c. Traduit de l'Allemand par M. Marcel de Serres, Ex-Inspecteur des Arts et Manufactures, et Professeur de la Faculté des Sciences à l'Université Imperiale; de la Societé Philomatique de Paris, &c. Paris, 1813.

(*Continued from p. 377.*)

From the statements given in the second chapter of his work, of which an abstract will be found in the last number of the *Annals*, M. Œrsted draws the following conclusions:

1. That the force of combustibility and the burning force are the ultimate chemical forces to which our experiments conduct us.

2. That they are likewise the forces which give to bodies their physical properties.

3. That we may consider these forces as the primitive and universal forces of bodies.

Chap. III.—*Of the Action of Forces in the Chemical Circle.*

Our author gives the name of *chemical circle* to what is usually termed the galvanic or voltaic circle; because he is of opinion that the phenomena of that circle are produced by the two chemical forces of combustibility and burning; of which he treated in the second chapter. When two metals differing in their combustibility are placed in contact, and the circle completed by a quantity of water, the burning force of the water is attracted by the force of combustibility of the most combustible metal; while the force of combustibility of that metal repels the force of combustibility of the water, which force is attracted towards the least combustible metal, which contains an excess of burning force. Hence a current of the burning force of the water passes through the liquid towards the most combustible metal; while an opposite current of the force of combustibility passes through the same liquid towards the least combustible metal. Hence the hydrogen of the water is attracted to the least combustible metal, and the oxygen to the most combustible metal. The hydrogen, when it comes in contact with the least combustible metal, makes its escape in the form of gas; but the oxygen usually combines with the most combustible metal. Whatever promotes the action of the metals on the water increases the energy of the chemical circle. Hence water containing acids in solution answers best. The energy of the circle is increased by every repetition of the pair of metals with the liquid between each, because every pair of metals adds its own energy to that of the other. Alkalies are attracted to the same metal as hydrogen, in consequence of their excess of combustibility; while acids, for the contrary reason, are attracted to the same metal as the oxygen. It is obvious that all the other phenomena of the voltaic circle may be explained on the same principles.

The reader who is acquainted with Mr. Donovan's mode of explaining the action of the voltaic battery, will perceive a considerable resemblance between his hypothesis and that of Prof. Œrsted; though certainly there is a considerable difference between the two. Our author's view of the subject is less encumbered with hypothesis than that of Mr. Donovan; and he does not suppose the transfer of the chemical attractions and repulsions which constitutes the foundation of Mr. Donovan's hypothesis, and which is a supposition very difficult to conceive or to admit.

Chap. IV.—*Of Electric Forces considered as Chemical Forces.*

Electricity exhibits two forces of such a nature that they destroy the activity of each other. These, therefore, are truly opposite forces. One of these has received the name of *vitreous* or *positive* electricity, while the other is called *resinous* or *negative* electricity. Each of these [illegible] as a repulsive activity for itself, and an attractive [illegible] force. Hence they are capable of retaining [illegible] it is no longer possible to perceive any ext[illegible] sence. A body may even contain an immens[illegible] ypes our senses; but if we place such a body near a substance in a state of excitement, the attraction of the preponderant force in this last for the opposite force in the body and the repulsion which it exercises on the same force in the body will disturb the equilibrium, and will occasion an excess of positive force in one part of the body, and an excess of negative force in another part, leaving a zone between them in which the two forces are in equilibrium. If the excited body be removed, the equilibrium will be restored by the mutual action of the forces on each other. This fact that bodies become electric when brought into the neighbourhood of an excited body, which holds universally, demonstrates that every body contains the two electric forces, though in a latent state, in consequence of their mutual attractions.

When the body whose electrical forces are thus disturbed is brought still nearer the excited substances, the opposition of its forces augments considerably the portion nearest the excited body acquires more and more of the opposite electricity; while the portion further distant from it acquires more and more of the same kind of electricity. When the distance is diminished to a certain point, which varies according to circumstances, the electricity of the second body, which is attracted by that of the first, unites with it, and disappears at the same time; so that nothing remains but a portion of the electricity of the first body, and the electricity of the same kind accumulated in the most distant part of the second body. Only the same kind of electricity now remains in these two bodies. This has made electricians regard the process as the communication of electricity from the first body to the second.

It is obvious from these facts and many others stated by Prof. Œrsted that when electricity is accumulated in a body, it occasions the accumulation of the opposite kind of electricity in the zone next it by attraction, and the accumulation of the same kind of electricity in the zone next in succession by repulsion. It attracts the opposite electricity, renders it latent, while it is itself rendered latent at the same time. The second zone occasions a new zone of opposite electricity, which render each other latent in the same manner. Thus electricity is always propagated in an *undulating manner*. These changes succeed each other so rapidly in good conductors that we cannot observe them; but in bad conductors we can distinguish by means of the electrometer alternate zones of positive and negative electricity. Thus the transmission of electricity is merely a change in the equilibrium of the natural forces of bodies.

The transmission of electricity depends upon its *intensity* and upon its *quantity*. The intensity is measured by the greatness of the attractions indicated by the electrometer. The quantity may be measured by the surface charged with electricity up to a certain electrometrical degree. Other things being the same, the quantity of electricity is proportional to the surface; but the intensity is inversely as the space over which a certain quantity of force is spread.

The greater the intensity of electricity the more easily must it spread itself in space. Hence an electricity infinitely weak would be isolated by all the bodies in nature; because no body is a perfect conductor. The greater the quantity of electricity the more difficult is its complete transmission. The complete transmission becomes also more and more difficult the worse a conductor the body is through which it has to pass. Hence, however different the conducting power of two bodies may be, we can always find two quantities of electricity such that they will be transmitted by the two bodies in the same time.

If we wish to act chemically on different bodies, we must give them a quantity of electricity proportional to their conducting power, with an intensity inversely as that power. The electric spark exhibits the smallest quantity of electricity with the greatest intensity. In the Leyden phial, and still more in the electrical battery, the quantity of electricity is greater relative to its intensity. When electricity is communicated by contact, the quantity is often great, but the intensity very weak.

These principles are well exemplified in the action of electricity on water. When a current of electricity from a machine is made to traverse water by means of two opposite wires, the water is not decomposed, because the whole electricity is conducted by the liquid. But if we cover the wires with glass, except their very extremities, the whole of the electricity cannot be conducted by the water, and in consequence that liquid is decomposed. When water is made a part of the voltaic circle, the intensity is so small, and the quantity so great, that it is not wholly conducted; and hence the decomposition of water by the voltaic battery.

Indeed all the oxidations and deoxidations, the attractions of the opposite conductors for the acids and alkalies, &c. show, that the chemical and electrical actions are produced by the same forces.

CHAP. V.—*Of the Production of Heat and its Laws.*

This chapter is one of the most important in the whole work, as its object is nothing less than to give a *new theory of heat*. At the same time I must acknowledge, that the views of the author on this subject possess a certain degree of obscurity through which I am not quite certain whether I have been fortunate enough to penetrate. Whether this be owing to Professor Œrsted not having expressed himself with sufficient clearness, or to my too little acquaintance with the *dynamical theory* of chemistry, on which his reasoning depends, I cannot pretend to say; but I have read over the chapter three times without being able to see clearly the validity of the consequences which he draws from his theory, or even to form a very precise conception of the theory itself. His reasoning is inductive, and I shall endeavour to lay the different steps of the induction before my readers.

1. When electricity passes with facility through any body, there is no perceptible heat evolved; but heat is always produced when the electricity passes with a certain degree of difficulty, provided it does pass. And the more difficult the passage is, the greater is the degree of heat which appears. If we take a wire of a given diameter, and cause an electrical shock to pass through it, no heat will be produced; but by diminishing the diameter of the wire continually, and still transmitting the same quantity of electricity through it, we shall find that it will become hot, then red-hot, and that it will finally be dissipated in fumes. Now the difficulty of the passage of the electricity obviously increases in proportion as the diameter of the wire diminishes.

2. The better a conductor a metal is, the more difficult it is to fuse it by electricity. Thus copper, which conducts electricity better than iron, is much more difficult of fusion by electricity than iron; so great is the difference that no electrical shock that we can produce is capable of fusing more than a very small portion of copper wire; while the same shock is capable of fusing a length of iron wire of the same diameter, amounting to six or eight feet. The reason why zinc, lead, tin, &c. are not so easily fused by electricity as by heat is obviously the goodness of these metals as conductors, which prevents the requisite degree of heat for fusing them from being evolved.

3. When a galvanic current is made to pass through a tube filled with water, we shall find that the greatest quantity of heat is evolved in the middle of the water, and a thermometer placed at the negative extremity acquires the least heat. This was proved by a set of experiments made on purpose by Œrsted, and by another set by M. Buntzen. In Professor Œrsted's experiments, the water was contained in a tube composed of sealing-wax; in those of M. Buntzen the water was confined in a glass tube. The reason of the different quantities of heat which appear in different parts of the water seems to be this; that in the middle of the water no gas whatever is evolved, while at the negative end there is an evolution of a greater bulk of gas than at the positive end. This gas must deprive the water near it of a portion of its heat.

4. Thus it appears that heat is evolved when electricity is transmitted by contact; a fact which seems inconsistent with the hypothesis of those who make the heat produced by electricity to depend upon a mechanical vibration; for the vibration cannot in such a case be great; and we know how difficult or impossible it is to produce heat by the friction of fluids against each other. The simultaneous disengagement of gas and heat does not seem to accord with the theory of heat at present

received. We may likewise heat wires by the electricity of contact. To produce this effect, we must employ large metallic plates, in order to collect a great quantity of electricity relative to its intensity.

Such is the induction, from which Prof. Œrsted concludes that it is a general law that *bodies become* HOT *whenever they are forced to conduct a greater quantity of electricity than they can freely transmit.* In such cases there is always a considerable accumulation of the opposite electricities before they unite. It is this union of the two electricities which produces heat. Professor Œrsted does not explain himself with regard to the nature of heat. He does not inform us whether it be a *substance* formed by the union of the two electricities, as was the opinion of Winterl. Indeed from his mode of expressing himself, he seems rather to be of opinion that the two electricities are not substances, but forces; and that heat is a force composed of the two opposite electrical forces united together. If this be the nature of his theory of heat, I confess that I am unable to form any accurate conception respecting it. I can conceive two opposite forces rendering each other *insensible* by their mutual action; but I cannot conceive them to unite together, and form a new force of a different nature. I can form a conception of what Winterl means when he says, that heat is a *substance* composed by the union of the two opposite electricities, provided these electricities be substances; but if they are merely opposite forces, the affirmation that heat is produced by their union seems to me at least to be merely words destitute of ideas attached to them.

As I am unable to perceive the accuracy of the reasoning in the following paragraph, in which Prof. Œrsted deduces some of the most striking particulars respecting heat as consequences of his theory, I shall present the reader with a literal translation of the passage.

"This action (*the union of the two electricities*) ought, therefore, to disappear in a point of space just when it begins to act in the succeeding point. Accordingly it leaves no trace as long as it meets with no obstacle; but when it meets with resistance, the case is quite different. The force which ought to accumulate in the place where the obstacle occurs, not being at liberty to put itself in equilibrium with an opposite force in the prolongation of the line, turns its action towards another point, where there is less resistance, in order to continue to act in the same manner. This is what happens in the reflection of heat. The new direction will be determined by the direction which it had before, and by that of the resistan [illegible] nay be determined by the fundamental principles of the [illegible] point out the law known to all the world that th [illegible] n is equal to the angle of incidence. It is ea [illegible] t we have deduced here from our principles ap [illegible] nt heat. We shall continue a little further the [illegible] orific action. It is obvious that heat ought to be better reflected by the surfaces which have a metallic lustre than by those which want that lustre; because this lustre indicates that the surface has little inequality, especially in the smallest parts. But we know likewise that the forces of which we are treating are transmitted more easily by means of points elevated above a surface than by those that form a level surface. Hence we see that bodies with a brilliant surface ought not merely to reflect more perfectly the external heat which endeavours to penetrate into them, but likewise the internal heat which endeavours to escape, as has been proved by the beautiful experiments of Leslie and Rumford. According to our principles, those bodies which are the least capable of conducting a great quantity of electric forces are the most proper to transmit this calorific action; for they are most proper for its production, and its propagation is merely a continued production. The small number of experiments to which we can at present apply this principle confirm it; especially the great facility which we find in all the gases for this sort of transmission. It would be requisite to ascertain whether the oils do not possess the same property in a higher degree than all other liquids."

The opposite forces in bodies are disturbed by friction. If one of these forces be permitted to make its escape by opening a communication with the earth, we have the phenomena of electricity; but when this separation does not take place, nothing takes place but an internal change in the equilibrium; and in consequence, the different phenomena of heat. Prof. Œrsted then shows at considerable length that the evolution of heat by friction is inconsistent with the common theory which supposes a calorific matter; but that it is perfectly consistent with his own theory that heat is the consequence of the union of the two electricities in particular circumstances.

The quantity of the opposite forces which exists in each body appears then to be very great. The chemical properties of the body depend upon the preponderating force; but the preponderating quantity must, in all cases, be very small, when compared with that of the forces which are in equilibrio. The dilatation of bodies is not owing to the preponderating force, but to the expansive property of those forces which are in equilibrium, and which produce a dilatation which is greater or less according to the intimacy of their combinations; for a body occupies the less volume the more intimately its forces are united, and is the more dilated the less intimately they are united. Hence a hot body in which the equilibrium of the forces is more disturbed ought to be more dilated than a cold body in which the equilibrium is more intimate.

The remainder of this chapter is taken up by our author in showing that all the facts respecting heat with which we are acquainted follow naturally as consequences from his theory. These facts are chiefly the following: 1. That all bodies contain heat; 2. That heat changes the state of bodies, converting solids into liquids, and liquids into elastic fluids; 3. That heat favours chemical combinations and decompositions; 4. That heat is disengaged in every case of chemical combination; 5. That bodies have different capacities for heat; 6. That when solids are converted into liquids or liquids into vapours, a certain quantity of heat always disappears. These phenomena are accounted for in a very simple and satisfactory manner. Indeed the only thing that our author's theory of heat wants is to be expressed in a more precise and definite manner.

(To be continued.)

Article VII.

Proceedings of Philosophical Societies.

LINNÆAN SOCIETY.

March 2.—A paper, by the Rev. W. Kirby, was read, on the characters of the Otocerus and Fulgorella, two new genera of Hemipterous insects belonging to the family of Cicadiadæ, with a description of several species.

Also an account, by Mr. J. Drummond, of some experiments made in the Cork Botanic Garden, by sowing the powder found in the ripe capsules of Funaria hygrometrica.

March 16.—Mr. Lindley's monograph of the genus *rosa* was continued.

April 6.—A paper, by R. Brown, Esq. was read, on Lyellia, a new genus of mosses, with some remarks on Leptostomum and Buxbaumia.

At this meeting there was also begun a memoir on the birds of Greenland, by Capt. E. Sabine.

A paper was likewise read, entitled "Remarks on the Changes of Plumage of Birds," by the Rev. W. Whitear.

April 20.—Capt. E. Sabine's account of the birds of Greenland was continued.

At this meeting was also read, an account, with drawings, of vegetable specimens found in the coal-pits in the neighbourhood of Camerton, by the Rev. Mr. Skinner.

Article VIII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Specific Heat of Fluids and Solids.*

M. Dulong is stated to have been led to conclude from his recent experiments on this difficult subject, that the specific heat *of all the simple gases is the same, and that the specific heat of simple solids is proportional to their capacity for oxygen.* These laws, if well founded, may be justly classed among the most important developed by modern chemistry.

II. *Euclase.*

Berzelius has lately subjected this beautiful and scarce mineral to a more accurate analysis than that of Vauquelin, who sustained a loss of 21 per cent. The result of Berzelius's analysis is as follows:

Silica	43·32
Alumina	30·56
Glucina	21·78
Oxide of iron	2·22
Oxide of tin	0·70
	98·58

From this analysis, Berzelius concludes that it is a compound of one atom of silicate of glucina and two atoms of silicate of alumina.

Vauquelin's analysis gave the following constituents of this mineral:

Silica	36
Alumina	23
Glucina	15
Oxide of iron	5
Loss	21
	100

III. *Iron Ore of the Isle of Elba.*

Crichtonite possesses a peculiar metallic lustre, which belongs also to the iron ore from the island of Elba. This circumstance led Berzelius to suspect the existence of titanium in this latter ore. On making the experiment, he found his conjecture verified. The presence of titanium in this ore indeed may be discovered by the blow-pipe. Dissolve before the blow-pipe a little of the Elba ore in the double phosphate of ammonia and soda, and then reduce it completely by exposing it to the interior flame of the blow-pipe. The colour of the iron disappears almost entirely during the cooling, and at the instant that the globule becomes solid there appears a reddish orange colour, which is owing to the presence of titanium or wolfram. Berzelius ascertained that in Elba iron ore it was titanium which was present.

IV. *Yellow Oxide of Uranium of Autun.*

From an analysis of this ore lately made by Berzelius, it follows that it is a uranate of lime containing a great deal of water of crystallization. That of Cornwall is the same combination coloured by arseniate of copper.

V. *White Pyrites, or Radiated Pyrites.*

It is well known to mineralogists that the crystalline forms of this variety of pyrites are quite different from those of common pyrites, and cannot be mathematically deduced from them. On that account, Haüy has constituted it into a peculiar species, under the name of *white pyrites.* Werner distinguished it under the name of *kammkies.* Berzelius has lately subjected it to a careful analysis, at the request of M. Haüy, to determine whether any difference existed in its composition, as had been inferred from the difference in its crystalline characters. But he has been unable to discover any distinction whatever between white pyrites and common pyrites. He proposes to repeat his experiments on other specimens. Does this mineral constitute an exception to the science of crystallization as seems to be the case with arragonite?

VI. *Phosphate of Manganese of Limoges.*

This mineral was discovered some years ago by Alluan, and sent to Vauquelin as an ore of tin. That celebrated chemist subjected it to analysis, and ascertained its composition. It has been lately subjected to a new analysis by Berzelius, who has found it a compound of one atom subprotophosphate of iron and one atom of subprotophosphate of manganese. The constituents extracted from it were as follows:

Phosphoric acid	32·8
Protoxide of iron	31·9
Protoxide of manganese	32·6
Phosphate of lime	3·2
	100·5

Vauquelin's analysis gave the constituents as follows:

Phosphoric acid	27
Oxide of iron	31
Oxide of manganese	42
	100

VII. *Fibrous Quartz.*

Mineralogists are well acquainted with a variety of quartz which Werner, from its texture, denominated fibrous quartz. Mr. Zellner, of Pless, lately analyzed a specimen of this variety of quartz, from Hartmannsdorf. He found its constituents as follows:

Silica	98·75
Oxide of iron	0·75
Water	0·25
	99·75

Its specific gravity was found 2·608.

VIII. *On the Discovery of Bipersulphate of Iron.*

By Charles Sylvester, Esq.

(To Dr. Thomson.)

SIR, *Derby, April* 5, 1819.

In the *Annals of Philosophy*, vol. xii. p. 462, you gave an account of the persulphates of iron, in which you stated your discovery of a quadripersulphate of iron, and you gave your opinion of the probable existence of several others, among which was included the bipersulphate. This latter salt I have been in the habit of preparing for the last seven years, and it has been used in a liquid form as a tonic mixture to a considerable extent during that period, particularly at Derby, Nottingham, and Sheffield. It has also been prescribed by my friend Dr. Robinson, Physician to the London Hospital.

When the solution (which was generally made to the same strength) by any accident became more than usually concentrated, a white pearly precipitate was always formed, giving the liquid the appearance of thick soap suds. This gave me first the idea that some crystallized compound was formed. On evaporating the clear solution I always had the same white precipitate, but did not for some time obtain the salt in regular crystals.

By spontaneous evaporation in a broad shallow vessel, I procured the salt in distinct crystals, which were octahedrons. I showed the salt in this state to a number of my chemical friends, about three years ago, and have made it in small quantities since that time

It was my intention to have published an account of it as soon as I had contrived an apparatus for making it with facility. This, however, I have been prevented from doing by my other engagements, and I should not at the present time have referred to the subject had it not been for the appearance of an account of this salt in the number of your *Annals* for April, by Mr. Thomas Cooper; in which he states that he has formed this salt in octahedral crystals, confirming your opinion of its probable existence.

It now becomes necessary for me to state a circumstance, which, however it may appear to charge Mr. C. with want of candour, I shall entirely acquit him of any improper motive.

In the latter end of last summer, I showed the salt in question to Mr. Cooper, telling him that it was a bipersulphate. The crystals were not large, but with a glass the octahedrons could be distinctly seen. I did not tell him the process, but the name would easily lead to it; since nothing more is necessary by the account he has given of it than to boil together sulphuric acid and the peroxide of iron in the proportions to make two atoms of acid to one of base. I am, your obedient servant,

CHARLES SYLVESTER.

IX. *Alloys of Platinum.* By Mr. Fox.

(To Dr. Thomson.)

ESTEEMED FRIEND, *Falmouth, Fourth Month,* 10, 1819.

Although it be well known that platina readily forms alloys with many of the metals, I am not aware that the phenomena which attend its combination with some of them have ever been noticed; and under this impression I shall briefly mention the result of some experiments I have recently made.

If about equal bulks of platina and tin be heated to redness, in contact with each other, they will combine suddenly with great vehemence, and a very considerable extrication of light and heat, which will continue for some little time after their removal from the fire.

This experiment may be easily tried with a blow-pipe, either by placing the metals together on charcoal, or enveloping them (or tin only) in platina foil, and exposing them to the flame from a blow-pipe at the end of some platina wire. The more effectually the free access of the air to the tin is prevented the better, as a very small degree of oxidation on the surface greatly diminishes the success and brilliancy of the experiment. When prepared in this manner, the flame of the candle alone, without a blow-pipe, is sufficient to fuse these metals, provided the quantity be not too great.

The moment the combination commences, the whole is formed into a brilliant globule of fused metal; and the heat is so intense that on my letting it drop into a basin of water, it continued a short time at a very high red heat under the water; and not only discoloured the part of the basin where it fell, but even imbedded itself in the glaze of the earthenware, so that it was not readily detached from it.

The same phenomena were exhibited in the combination of platina with antimony. The latter alloy I exposed for a considerable time to a high degree of heat, till it ceased to be in a state of fusion, in consequence of the sublimation of the antimony; when, on being hammered, it proved to be malleable; in fact, very little besides pure platina remained. In this manner, I am of opinion, platina may be obtained, if not quite pure, at least sufficiently so for all the common purposes to which platina is applied, as it certainly will not be fused again at any heat under that which has been employed in making it.

Zinc enveloped in platinum foil, so as to exclude the free access of the air, on being exposed to the flame from a blow-pipe, exploded with vivid combustion, and was wholly converted into the white oxide. Very little of the platina was fused.

I attribute the great heat excited by the combination of platina with tin and antimony to the inferior capacity for heat of the alloy, though this, perhaps, does not fully explain the cause of the very rapid combination that takes place.

The inflammation of the zinc is probably owing to its attaining a high degree of heat before it bursts the covering of platina; and its combination with the oxygen of the atmosphere is, therefore, instantaneous.

Robert W. Fox.

X. *New Principle in the Seeds of the Cytisus Laburnum.*

MM. Chevalier and Lassaigne have discovered the existence of a peculiar substance in the seeds of the cytisus laburnum, which possesses violent emetic properties. They obtained it by the following process. The seeds were boiled for some time in alcohol. The tincture thus obtained being filtered and evaporated to the consistence of an extract, the residue was digested in water. The aqueous solution was mixed with acetate of lead in order to precipitate an albuminous matter which it contained. A current of sulphuretted hydrogen gas was passed through the filtered liquid, in order to throw down the excess of lead which it contained. The liquid thus freed from albumen and lead was filtered and evaporated. What remained was the peculiar emetic principle of these seeds. Its properties were as follows:

Its taste was disagreeable. Eight grains of it swallowed at intervals occasioned vertigos, strong spasmodic contractions, flushing of the face, increased the velocity of the pulse, and occasioned violent vomiting. These symptoms lasted two hours, and left the person who had swallowed the substance in a state of considerable debility. Its colour is greenish yellow. It is not precipitated by acetate of lead, but it is by subacetate. It is precipitated by nitrate of silver, oxalate of ammonia, and muriate of barytes. Emetin, or the peculiar principle of ipecacuanha, is not precipitated by these last three reagents.

The other substances found in the seeds of the cytisus laburnum were the following:

1. A fatty matter of a greenish-white colour.
2. Albumen.
3. Green vegetable colouring matter.
4. Malic and phosphoric acids.
5. Malates of potash and lime.
6. Silica in very small quantity.—(Journ. de Pharm. Aug. 1818, p. 340.)

XI. *On Thermometrical Measurements of Heights, &c.*

By Mr. Murray.

(To Dr. Thomson.)

SIR, *Paris, March* 28, 1819.

I should be sorry to condemn an instrument ere its inutility had been positively decided; and when I consider its portability compared with the common mountain barometer, least of all, the thermometer of Wollaston for the determination of altitudes. I, therefore, write this rather with a view to excite observers to the repetition of experiment than to cast the apparatus into shade. I repeated the experiment of the ebullition of water on the summits of the Simplon and Mount Cenis, but the results were by no means *approximative*. The barometric measurement of the former may be questioned, but the latter I cannot hesitate to admit, confident of the accuracy of the observer (from a personal acquaintance with him), Dr. Frederick Schow.[*] The comparisons were made with the cotemporaneous observations of the Baron de Zack, at Genoa. As a question might, however, arise on the thermometers I employed, from their being without a minute graduation or a nonius, I shall beg simply to give you the result of the experiment made at the village of the Simplon on the evening of the 15th August last, with Capt. B. Hall. The bulb of the thermometer maintained in contact with the vapour of water in ebullition indicated a temperature of 201·6°, corresponding with 24·45 inches of the barometer, and equal to 5,400 feet altitude, being an excess of 577·75 feet above the barometrical height observed. Moreover, there should be a compensation for the expansion of the thermometric bulb; and if it be true, agreeably to the experiments of M. Gay-Lussac[†] (though these have been questioned in some recent researches), that water boils at a lower temperature in metallic than in glass vessels, some note should be taken of this, and these circumstances would *increase the excess*. I think a *metallic* scale from its expansion, unfavourable to accuracy of result.

I can conceive two causes operating against the results we would willingly accept. One of these is an *interim change of density* in the atmosphere. It would then require the compensation of the barometer, an instrument it was intended to supplant. A series of experiments should, therefore, be made to indicate on the level of the sea the points of ebullition for the barometrical range, and the degree of ebullition on the given height again compared with contemporaneous observations on the level of the sea, or other well ascertained position as noted by the barometer. This would not, however, account entirely for so enormous a difference which, on the great St. Bernard, amounted to about 1000 feet. The *hygrometric state* of the medium strikes me as the *chief* cause. Dew forms only in the valley, not on the mountain top; and experiment has amply confirmed the extreme siccity of the atmosphere at great elevations. By providing the rarified medium with an absorbing material, Professor Leslie has ingeniously and beautifully effected the congelation of water. By a parity of reasoning, we infer the converse of this, and which I shall forthwith make the subject of experiment. Under such circumstances, the march of the hygrometer must be noted, and this instrument will become an indispensable accessary. At inconsiderable heights, this will not make a notable difference; but at great altitudes the amount will be material. Hence the results of the Rev. Mr. Wollaston's interesting researches.

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[*] Dr. Schow is one of the scientific gentlemen employed by order of the King of Denmark. His department is the determination of the geographical position of plants, so ably begun by Baron de Humboldt. To his unwearied and mitting exertions I cheerfully pay my tribute of admiration.

[†] Any opposition to the experiments of this very acute observer received with all due caution.

I should like to know whether the *temperature of snow* has been observed to maintain any ratio with its altitude. In crossing the Boeketta, I found the temperature of the snow to be 30° Fahr. On the 8th of January last, at 10 o'clock, a.m. the snow about 1500 feet lower than the station of the Grande Croix, on Mount Cenis, was 22° Fahr. At the Grande Croix at 11h 30′, a.m. the temperature of the atmosphere was 22°, while that of the snow was 21° Fahr. The question is at least interesting.[*]

It has, I know, been presumed, that animal heat continues uniform in an exaltation of temperature; and Mr. Brodie has ascribed its production to the operations of the brain, which had hitherto been supposed referable to the action of the lungs; but Dr. John Davy found a difference of about one degree of excess in tropical climes.

In the Stuffa San Germano, on the border of the Lago Agnano, near Naples, I found the animal temperature to be 102° Fahr.; the medium was 110°. I remained here *nearly half an hour* to examine chemically the nature of this subterranean vapour, so that the excess cannot be entirely ascribed to a sudden transition. The thermometer held in the aperture through which the sulphureous vapour entered, exhibited a temperature of not less than 160° Fahr.[†] It enters by *jerks*, which was well demonstrated by corresponding oscillations of the mercury in the thermometer. The air in the chamber was *extremely dry*, and Leslie's hygrometer[‡] completed its range of 80°. I think I have not been deceived in noting the animal temperature on the Simplon and Mount Cenis at a *decrement of several degrees*.

I have the honour to be, Sir,
Your very humble and most obedient servant,
J. Murray.

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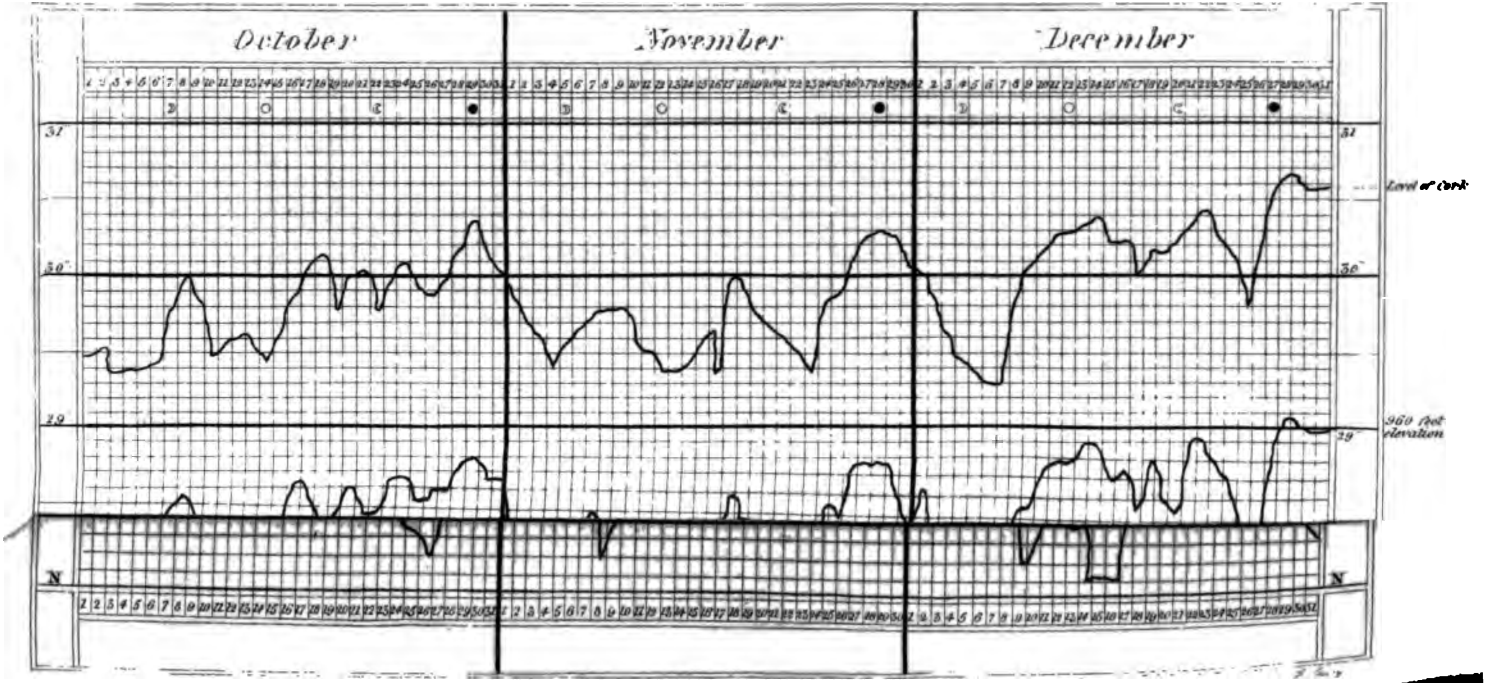
[*] From the nonconducting powers of snow with respect to caloric, it might at least afford an equable mean of determining the medium temperature on given altitudes.

[†] I remember Mr. Davenport's experiments on boiling tar interested me at the time. I placed my hand in the elevated temperature for some time without suffering the least inconvenience, and so long as the epidermis was *free from moisture.*

[‡] I have found this instrument among the Alps a most valuable acquisition. In the morning before I pursued my journey, if there was no evaporation indicated, or only a few degrees of fall, I could always *conclude with confidence* on *rain* during the day.

Barometer


October
November
December
31
30°
29
31
30°
29°
Level of Cork
960 feet elevation
N
N

I. *Meteorological Observations at Cork.* By T. Holt, Esq. (With a Plate. See XCIII.)

(To Dr. Thomson.)

SIR, *Cork, Feb.* 3, 1819.

TRANSMIT you the meteorological scale and journal for t, kept during the last quarter of 1818; as also a summar y observations for the whole year.

I have the honour to be, Sir, with due respect,
Your very obedient humble servant,
THOMAS HOLT.

REMARKS.

OCTOBER.

air; shower.
listy; rain.
air.
, 6. Fair; some showers.
air.
air.
air; cloudy.
howery.
howers of hail; windy.
ale; heavy rain.
. Showery; windy.
air; occasional showers.
. Bright days.
itto; windy.
loudy; rain.
howery; dense fog.
ry; cloudy.
ain.
, 25, 26. Dry, cloudy days.
ainy.
ry; cloudy.
air; some showers.
owery.
air.

NOVEMBER.

Dry; cloudy.
air; rainy evening.
og; fine day.
right; frost.
rost; fair and windy.
Dry; overcast.
, 11, 12. Rainy.
owery.
air; occasional showers.
air; rainy evening.
. Fair.
isty.
isty; heavy rain and wind.
owery.
21. Fair; some showers.
22. Fair; heavy gale, with rain.
23. Fair; some showers.
24. Bright day.
25. Showery, with wind.
26. Fair.
27. Showery.
28. Misty; overcast.
29. Dry; cloudy.
30. Showers.

DECEMBER.

1. Showers.
2. Frost; clear; wind.
3. Frost; fair.
4. Dry; cloudy.
5. Frost; showery; wind.
6. Fair; rain and wind.
7. Showery.
8. Bright.
9. Dry; cloudy.
10, 11, 12. Cloudy; breeze.
13. Cloudy; light showers.
14. Bright; frosty night.
15. Ditto; rainy evening.
16. Bright.
17. Cloudy.
18. Heavy rain and wind; clear afternoon.
19. Showery.
20. Dark, misty day.
21. Dense fog in Cork; but clear and bright on the hills.
22, 23. Misty; light showers.
24. Dry; cloudy.
25. Showers; rain and wind.
26. Showery.
27. Dry; cloudy.
28. Clear day.
29, 30. Dry; cloudy.
31. Fair; bright.

RAIN.

1818.	Inches.	1818.	Inches.	1818.	Inches.
Oct. 1	0·012	Nov. 3	0·291	Dec. 1	0·200
2	0·371	9	0·177	5	0·429
4	0·141	10	1·047	6	1·104
5	0·096	11	0·309	7	0·438
6	0·078	12	0·168	13	0·058
10	0·261	13	0·660	15	0·096
11	0·261	14	0·189	18	1·126
12	0·891	15	0·360	19	0·045
13	1·152	19	0·015	22	0·080
14	0·453	20	0·075	23	0·016
15	0·459	21	0·036	25	0·576
19	0·426	22	0·846	26	0·144
20	0·126	23	0·072		
22	0·267	25	0·114	Dec....	4·362
27	0·019	27	0·123	Nov....	4·627
29	0·024	30	0·145	Oct....	5·126
30	0·096				
			4·627		14·115
	5·126				

SUMMARY OF 1818.

Barometer (hill) Highest point, Dec. 28. Wind E.S.E. .... 29·08 in.
Lowest point, March 5. Wind S.W. .... 27·55
Mean of 365 observations .... 28·46
(town) Highest, Dec. 29. Wind E. .... 30·67
Lowest, March 5. Wind S.W. .... 28·00
Mean of 365 observations .... 29·48
Thermometer. Highest, July 16. Wind S.W. .... 80·00°
Lowest, Feb. 4. Wind W.S.W. .... 17·00
Mean of 533 observations .... 49·43
Rain in 1818 .... 38·037 inches.

Wind.	Days.
E.	56
W.	69
S.	49
N.	19
S.E.	23
S.W.	62
N.E.	23
N.W.	48
Var.	16
	365

Prevailing winds S.W.

	Days.
Bright days	118
Dry, cloudy	79
Showery	99
Rainy	52
Variable	17
	365

XIII. *Inquiry respecting a Meteoric Phenomenon described by Dr. Clarke.*

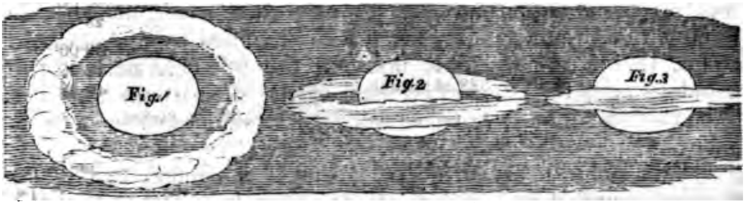
(To Dr. Thomson.)

SIR, *London, Feb.* 22, 1819.

In Dr. E. D. Clarke's amusing volume of travels in Scandinavia, a phenomenon is described which seems rather to border upon impossibility. As the author is an occasional contributor to your pages, he will, perhaps, be kind enough to furnish a fuller explanation of the passage than appeared to him necessary in a work intended for general circulation. I am far from wishing to insinuate that the Doctor allows himself the traveller's licence, but cannot help observing that the circumstance, as it is stated in his work, is calculated to try the faith of his readers to the utmost.

The passage to which I allude is the account of the appearance of the moon,* as observed by him on the road from Tornea to Kiemi, at p. 487. After having described the oval appearance of the moon's disc, he proceeds:

"This changeful scenery still continued, varying at every instant: at last there ensued a more remarkable appearance than any we had yet witnessed. The vapours dispersed, and all the rolling clouds disappeared, excepting a belt collected in the form of a ring, highly luminous, around the moon, which now appeared in a serene sky, like the planet Saturn augmented to a size 50 times greater than it appears through our best telescopes. The belt by which the moon's rays were reflected, became, beyond description, splendid, and the clear sky was visible between this belt and the full fair orb which it surrounded. Certainly if the same phenomenon had been visible in England, the whole country would have been full of it from one end of our island to the other."



In reading this passage nothing remarkable is observed; for it is easy to conceive that a circle of clouds may have been formed through which the orb of the moon was visible, as represented in fig. 1.

But as descriptions in words whether "demissa per aurem" or on paper

> "Segnius irritant animos
> Quam quæ sunt oculis subjecta fidelibus"

(viz. drawings); and as Dr. Clarke has accompanied his description with a wood-cut, which places the matter in a point of view entirely different, I intend on this cut to found my objections.

Figure 2 is a copy of the representation which the Doctor has given of this phenomenon. Now it is evident that to produce this appearance, the eye (which in fig. 1 is supposed to view the cloud in a direction perpendicular to the plane of the circle) must be so placed that the visual rays may be acutely inclined to the plane of the cloud; also that the cloud, if I may be allowed the use of an expression so awkward, must pass quite round on the other side of the moon; that is, on the side at the greatest distance from the earth; and this I hold to be impossible on the following grounds: Wherever there are clouds, there must be an atmosphere to support them. The atmosphere of the earth does not reach to the moon; and even allowing that it did, the clouds which surround this planet do not extend so far from its surface as the orbit of the moon is distant. The cloud observed did not exist in the atmosphere of the moon, inasmuch as no telescope that I ever heard of could detect such things, and this appearance was perceptible to the naked eye. From these considerations, we may, I think, infer, that the cloud did not exist on the further side of the moon; and, therefore, that there could be no such appearance as that described by Dr. Clarke.

---

* The application of the term "planet" to the moon, at p. 485, is, I think, of doubtful authority, and should have been rejected by the philosophic Clarke. According to this new nomenclature, the satellites of Jupiter, of Saturn, and of Uranus, are all planets; however, "de minimis non curat lex."

Let us now consider what would have been the appearance of the cloud, supposing it actually to encircle the moon in the direction of the horizon. The moon, as we may judge from Dr. Clarke's representation, was at the full, and the luminous appearance of the cloud was derived from her light. But the half of the moon most distant from the earth would not, in this situation of circumstances, have been illumined by the sun, and could, therefore, reflect no light on the adjacent part of the nebulous circle. The further half of the ring would thus not have been luminous; in other words, it would have been invisible.

I have only further to observe, that if the moon had really been surrounded by a circle of cloud, and if it had been so viewed that the plane of the cloud produced passed through the eye, the appearance would have been as in fig. 3. Now nothing is more common than to see a strait band of cloud, such as in Mr. Howard's nomenclature is called a *Cirrostratus*, stretching across the disc of the moon. Such an appearance is described by Dr. Clarke himself, at p. 485, where he says, that the moon appeared as if divided into two parts. We are perfectly familiar with such appearances in this climate, yet nobody ever supposed that they are caused by circles of cloud viewed in a particular direction. Indeed if this is the case with the Doctor, he has made a most unwarrantable assumption, the grounds of which he can best explain. If this was not the appearance of the moon, as viewed by Dr. Clarke, I can only account for the phenomenon by supposing it such as is represented by fig. I. In either case, the Doctor must have taken the liberty of supposing the point of view altered in order to produce the appearance of Saturn, with a representation of which he has treated his readers in fig. 2.

If you do not consider the matter as too trivial to occupy a place in the *Annals*, I shall feel obliged by your affording me an opportunity of meeting with an explanation, and am, Sir,

Your most obedient servant,

S.

XIV. *Notice of an Annular Eclipse in the Thirteenth Century.* By the Rev. James Yates, M.G.S.

(To Dr. Thomson.)

SIR, *Birmingham, May*, 1819.

The learned and curious observations of Mr. Francis Baily upon the annular eclipse of the sun, which will take place in September, 1820,[*] induce me to think that the following notice of one, which was seen in the same quarter of the globe, may be interesting to some of your readers. The passage occurs in "The Norwegian Account of Haco's Expedition against Scotland," first published in the original Islandic, with a literal English version, by the Rev. James Johnstone, A.D. 1782. I extract both the original Islandic, and Mr. Johnstone's English translation.

"Þá er Hakon Konongr lá i Rögnvalzvagi dró myrkr mikit á sólina, sva at lítill hringr var biartur um sólina utan, ok hellt því nockora stund dags."

Id est,

"While King Haco lay in Ronaldsvo a great darkness drew over the sun, so that only a little ring was bright round the sun, and it continued so for some hours."—P. 44, 45.

Haco invaded Scotland in the year 1263; he sailed with his navy into Ronaldsvo, which appears to have been the name of a bay or harbour in South Ronaldsay, one of the Orkney islands, some time "after St. Olave's wake;[†]" and he quitted Ronaldsvo "on the day of St. Lawrence's wake."[‡] These two days correspond to July 29 and August 9, which fixes the time of the eclipse with considerable precision, and shows it to be the same, which is marked in catalogues as having happened on Aug. 5, 1263. Had this account been published at an earlier period, it might have supplied in some degree the long chasm remarked by Maclaurin, who says, that Ricciolus in his Catalogue mentions no *annular* eclipse from the year 334 to 1567.[§] In the last edition of the "*Art de verifier les Dates*," A.D. 1783, this eclipse is marked as annular. The expression "it continued so for some hours," must be understood to mean only, that the obscuration of the sun continued for some hours.

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[*] *Annals of Philosophy* for Sept. and Oct. 1818.

[†] Norwegian Account, &c. p. 43.

[‡] Page 47.

[§] Phil. Trans. vol. xi. p. 193.

Article IX.

Magnetical, and Meteorological Observations.

By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 42″ North. Longitude West in time 1′ 20·7″.

Magnetical Observations, 1819. — *Variation West.*

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
April 1	8h 40′	24° 32′ 55″	1h 20′	24° 44′ 05′	6h 15′	24° 34′ 28″
2	8 40	24 31 34	— —	— — —	— —	— — —
3	8 40	24 30 32	1 20	24 43 05	6 20	24 34 47
4	8 40	24 34 21	1 50	24 42 21	6 15	24 35 59
5	8 40	24 30 52	1 25	24 43 56	— —	— — —
6	8 35	24 32 57	1 30	24 43 35	6 15	24 37 36
7	8 35	24 35 26	1 25	24 43 28	6 10	24 34 13
8	— —	— — —	— —	— — —	6 20	24 35 27
9	8 40	24 31 42	1 55	24 41 06	6 25	24 36 04
10	8 40	24 31 22	1 15	24 45 04	— —	— — —
11	8 40	24 30 48	1 35	24 50 08	6 25	24 36 30
12	8 50	24 34 29	— —	— — —	6 25	24 33 13
13	8 40	24 35 54	1 20	24 42 20	6 25	24 34 25
14	8 40	24 32 56	1 20	24 43 02	6 30	24 36 04
15	8 40	24 34 56	1 15	24 43 42	6 35	24 33 08
16	8 40	24 32 53	1 15	24 41 35	— —	— — —
17	8 45	24 31 38	1 25	24 42 20	6 25	24 36 16
18	8 40	24 29 24	1 45	24 41 45	6 35	24 34 25
19	8 40	24 31 19	1 15	24 41 25	6 35	24 34 28
20	8 40	24 31 56	— —	— — —	6 25	24 34 11
21	8 40	24 31 24	1 10	24 41 49	6 40	24 33 37
22	8 40	24 33 42	1 25	24 46 05	6 45	24 34 55
23	8 40	24 35 48	1 20	24 43 57	6 50	24 34 50
24	8 40	24 37 08	1 25	24 42 23	— —	— — —
25	8 40	24 34 22	1 35	24 42 53	6 55	24 34 53
26	8 45	24 31 33	1 30	24 45 09	6 55	24 32 18
27	8 45	24 32 38	1 20	24 42 07	6 55	24 35 56
28	8 40	24 30 35	1 25	24 42 40	6 55	24 34 18
29	8 40	24 31 56	1 25	24 42 04	6 55	24 35 42
30	8 40	24 32 19	1 25	24 39 45	6 55	24 36 58
Mean for the Month.	8 40	24 32 36	1 27	24 43 09	6 34	24 34 59

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
April		Inches.				Feet.		
1	Morn....	29·700	51°	50°	W by S		Fine	47•
	Noon....	29·710	60	40	W by S		Fine	61
	Even	29·710	56	47	W		Fine	46
2	Morn....	29·703	51	44	W		Fine	
	Noon....	—	—	—	—		—	62
	Even	—	—	—	—		—	47
3	Morn....	29·582	52	43	SW by S		Very fine	
	Noon....	29·548	60	37	W		Very fine	63
	Even	29·546	58	36	NNE		Very fine	43½
4	Morn....	29·686	47	55	ESE		Cloudy	
	Noon....	29·683	58	46	SE		Cloudy	59½
	Even	29·653	54	45	NW		Cloudy	42
5	Morn....	29·622	48	49	N		Very fine	
	Noon....	29·617	54	37	NE		Very fine	53½
	Even	—	—	—	—		—	37
6	Morn....	29·453	44	44	ESE		Fine	
	Noon....	29·345	53	38	E		Fine	53
	Even	29·265	47	42	E by S		Cloudy	41½
7	Morn....	29·200	46	45	E by S		Very fine	
	Noon....	29·243	59	35	ESE		Very fine	61½
	Even	29·270	56	41	ESE		Rain	48½
8	Morn....	29·388	—	69	WNW		Rain	
	Noon....	—	—	—	—		Rain	53½
	Even	29·458	52	78	NW		Fine	40½
9	Morn....	29·552	48	52	N by E		Very fine	
	Noon....	29·565	54	47	NW		Cloudy	55
	Even	29·570	52	45	N by W		Cloudy	39
10	Morn....	29·560	47	45	N by W		Very fine	
	Noon....	29·463	56	40	Var.		Very fine	60
	Even	—	—	—	—		—	41
11	Morn....	28·973	46	77	W by S		Rain	
	Noon....	28·910	54	44	WSW		Showery	56½
	Even	28·871	51	44	SSW		Cloudy	40½
12	Morn....	28·850	43	70	ENE		Rain	
	Noon....	—	—	—	—		—	47
	Even	28·812	47	99	NE		Mizzle	47
13	Morn....	28·760	50	87	E		Rain	
	Noon....	28·800	47	65	W		Cloudy	53½
	Even	28·931	47	54	SW		Thunder	40
14	Morn....	29·020	47	56	S by W		Fine	
	Noon....	29·020	54	45	SSW		Hail	56
	Even	28·975	50	47	SSE		Fine	42
15	Morn....	28·964	48	51	WSW		Fine	
	Noon....	28·983	55	44	SSW		Fine	56½
	Even	28·970	50	47	SSE		Cloudy	44½
16	Morn....	28·634	48	65	S by W		Showery	
	Noon....	28·706	53	48	SW by S		Cloudy	55
	Even ...	—	—	—	—		—	42½
17	Morn....	28·900	45	56	SSW		Hail	
	Noon....	28·967	52	45	SSW		Showery	54
	Even	28·968	47	45	SSW		Show	
18	Morn....	29·162	47	43	WSW		Very	
	Noon....	29·210	52	40	SW		Sho	
	Even	29·253	47	50	W by S		Sho	

Meteorological Observations continued.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
April		Inches.				Feet.		
19	Morn....	29·354	45°	51°	SW		Cloudy	36½
	Noon....	29·359	54	42	SSW		Cloudy	54
	Even	29·320	49	71	SSE		Rain	48
20	Morn....	29·238	53	25	WSW		Cloudy	
	Noon....	—	—	—	—		—	58
	Even....	29·225	54	58	SW		Showery	45½
21	Morn....	29·179	50	66	SW by W		Showery	
	Noon....	29·200	52	60	W		Showery	57½
	Even	29·243	49	45	WNW		Fine	41
22	Morn....	29·375	45	70	N		Rain	
	Noon....	29·430	45	59	NE		Cloudy	47½
	Even	29·430	45	58	NE		Cloudy	42
23	Morn....	29·324	45	61	ENE		Rain	
	Noon....	29·267	48	51	E		Cloudy	49½
	Even ...	29·217	45	55	E by N		Showery	44½
24	Morn....	29·103	47	88	E		Rain	
	Noon....	29·105	47	75	ENE		Rain	47½
	Even	—	—	—	—		—	43½
25	Morn....	29·256	44	85	ENE		Rain	
	Noon....	29·368	44	73	ENE		Rain	47
	Even	29·478	43	53	E		Fine	34
26	Morn....	29·660	46	42	E by N		Fine	
	Noon....	29·671	51	33	E		Fine	53
	Even....	29·671	43	41	E by S		Clear	35
27	Morn....	29·684	45	34	ESE		Fine	
	Noon....	29·700	48	31	ESE		Very fine	50
	Even....	29·730	42	34	E		Clear	36½
28	Morn....	29·753	46	33	SE		Hazy	
	Noon....	29·753	54	28	E by S		Fine	56½
	Even	29·730	47	32	E by S		Fine	39½
29	Morn....	29·600	50	34	ESE		Very fine	
	Noon....	29·582	52	27	SE by E		Very fine	56½
	Even	29·520	46	34	E		Very fine	38½
30	Morn....	29·469	47	31	S		Fine	
	Noon....	29·446	55	25	SE		Fine	59
	Even	29·410	50	27	SSW		Very fine	

Rain, by the pluviameter, between noon the 1st of April and noon the 1st of May 2·468 inches. Evaporation during the same period 3·44 inches.

ARTICLE X.

METEOROLOGICAL TABLE.

1819.	Wind.	Barometer.			Thermometer.			Hygr. at 9 a. m.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
3d Mo.										
Mar. 19	Var.	29·50	29·21	29·355	51	38	44·5	90	16	●
20	N W	29·75	29·50	29·625	46	37	41·5	65	—	
21	N W	29·80	29·75	29·775	48	32	40·0	59		
22	N W	29·80	29·70	29·750	53	35	44·0	63		
23	S	29·70	29·49	29·595	51	42	46·5	61	—	
24	S W	29·62	29·49	29·555	58	44	51·0	77	—	
25	S W	29·85	29·64	29·725	55	37	46·0	67	—	☽
26	W	29·90	29·85	29·875	54	40	47·0	68	—	
27	S W	29·90	29·73	29·815	55	46	50·0	59	59	
28	S W	29·77	29·68	29·725	54	46	50·0	85	—	
29	S W	29·96	29·67	29·815	57	43	50·0	67		
30	S W	30·07	29·96	30·015	58	50	54·0	69	8	
31	S W	30·18	30·07	30·125	59	47	55·0	69		
4th Mo.										
April 1	W	30·20	30·15	30·175	62	48	55·0	61		
2	W	30·15	30·05	30·100	68	36	52·0	66		○
3	N	30·17	29·99	30·080	68	43	55·5	67		
4	E	30·17	30·07	30·120	60	38	49·0	61		
5	N E	30·06	29·94	30·000	60	29	44·5	61		
6	S E	29·94	29·60	29·770	54	43	48·5	60		
7	E	29·80	29·62	29·710	66	46	56·0	60	10	
8	N W	30·00	29·80	29·900	58	40	49·0	74	33	
9	N	30·07	29·95	30·010	61	34	47·5	67		
10	W	29·95	29·35	29·650	64	40	52·0	66		☾
11	S W	29·35	29·27	29·310	60	37	48·5	66	14	
12	N E	29·30	29·12	29·710	50	43	46·5	74	35	
13	Var.	29·48	29·12	29·300	58	40	49·0	85	3	
14	S W	29·48	29·30	29·390	60	42	51·0	74	—	
15	S W	29·40	29·03	29·215	60	45	52·5	76	—	
16	S	29·40	29·03	29·215	58	44	51·0	68	45	
		30·20	29·03	29·738	68	29	49·20	68	2·23	

The observations in each line of the table apply to a period of twenty-four ours,beginning at 9 A. M. on the day indicated in the first column. A dash enotes, that the result is included in the next following observation.

REMARKS.

Third Month.—19. A moderate gale at SW in the early morning, with much cloud carried by the wind. About 10, the wind changing suddenly to NW, the whole mass of cloud to the southward became an immense *Nimbus*, the base reaching from the SW to the NE, with a lighter sky visible beyond: at the same time precipitation was going on overhead, and we had soon a smart shower mingled with hail: the whole ended in a uniform veil of *Cirrostratus*, and at night we had the SW wind again pretty strong. 20. The wind changed again to NW, a.m. with much cloud, and some drops of rain. 21. Fine day: a smart breeze from NW. 22. Fine day. 23. A trifling shower. 24. Wet, wind, morning: fair day. 25. A shower with hail at mid-day: a large *Nimbus* passed, and a distant peal of thunder was heard to the NW. 26. Chiefly *Cumulostratus*: a very little rain, p.m. 27. Windy, with much cloud, and two or three showers. 28. Cloudy: a gale through the day. 29. Cloudy. 30. A rainbow at nine, a. m.: squally, with showers: the bow again twice about three, p. m. 31. Cloudy: some drops of rain.

Fourth Month.—2. A lunar halo at night, of large diameter, and colourless: it was sensibly elliptical, the longer diameter being the perpendicular; it continued two or three hours. 3. Large *Cirri*, with *Cumuli*: much dew: very fine day. 4. *Cumulostratus*. 5. Fine morning: the hoar frost remained at seven, a. m. on some tufts of *Saxifraga cæspitosa*, &c. (as heretofore noticed) long after it had disappeared elsewhere in my garden; proving that the warmth which melted it came in great part from the earth. 6. Large plumose *Cirri*, with *Cirrostratus*, a.m. 7. The maximum of temperature for the past 24 hours at nine this morning: thunder-clouds ensued, which soon passed to a quiescent mixture of different modifications, and rain came on at evening. 8. Much *Cirrostratus*, with pretty heavy rain, p. m.: at evening the wind changed to NW, with a rainbow and a turbid mixture of different clouds. 9. Fine, with *Cumulostratus*: wind N, p. m. 11. The clouds this evening were tinged with a strong lake colour, on the bases of *Cumulostrati*, beneath *Cirrus*: some rain attended. 12. Wet, most of the day. 13. Rain, a.m. 14. *Cumulostratus*: in the evening streaks of *Cirrus* from SW to NE, followed by wind and rain. 15. Clouds followed by rain in the night, as before. 16. After a fine day with clouds, rain in the early morning.

RESULTS.

Winds chiefly Westerly.

Barometer:	Greatest height	30·20 inches.
	Least	29·03
	Mean of the period	29·738
Thermometer:	Greatest height	68°
	Least	29
	Mean of the period	49·20
Mean of the Hygrometer		68°
Rain		2·23 inches.
Evaporation		1·32 in.

Tottenham, *Fourth Month*, 20, 1819. L. HOWARD.

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ERRATA FOR VOL. XIII.

Page 216, line 4 (from bottom), *for* 556° *read* 656°.
3 (from bottom), *for* 580° *read* 680°.
227, line 2, *for* poles *read* equator.
377, line 5 and 10 from bottom, *for* Corbeth *read* Carbeth.
442, line 8 from bottom, *for* 2 atom *read* 1 atom.
443, line 20, *for* eight cubic inches of oxygen gas, *read* eight cubic inches of sulphurous acid gas.

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